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ARTICLES

CHANGE AND STABILITY IN SUPERPOWER RIVALRY

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We investigate the dynamics of superpower rivalry. Participants in policy debates within each state use information about expected future threats and economic costs to influence other policy actors, and this process of sophisticated reaction links the security policies of these two states into a single rivalry system. Analysis of vector autoregression models of U.S. and Soviet military expenditures and diplomatic hostility and U.S. gross national product supports the hypothesis that these policies approximate the behavior of unitary rational states capable of forming rational expectations of each other's future behavior. The dynamic response of this system to a wide range of exogenous shocks (or innovations) reveals the underlying stability of this rivalry system. The military expenditures of both states exhibit a cyclical response to innovations, with a shorter U.S. cycle. This lack of synchronization creates several problems for analysis and for policy change.

Although competition between the United States and the Soviet Union covers a wide range of diplomatic, military, and economic policy areas, nearly all quantitative analyses of U.S.-Soviet relations consider only a few of the many dimensions of superpower rivalry. To take the most prominent example, the arms race research program begun by Richardson (1960) uses different measures of military expenditures or military capability to test closely related models.[1] Yet the arms race cannot be studied in isolation but must instead be placed within the broader context of political competition between and within the superpowers.

We outline an integrated program of theoretical and empirical research focusing on the overall dynamics of superpower rivalry. We use statistical modeling techniques to examine the underlying structure of rivalry without imposing a specific model of the domestic political processes within each state. In contrast to the anomalous or inconsistent results that characterize previous studies of the arms race, our vector autoregression analysis reveals a considerable amount of consistency between the data and our theory. In fact, our data analysis reveals a remarkable stability—even simplicity—in the dynamics of superpower rivalry.

From Arms Race to Rivalry

Our research on U.S.-Soviet rivalry has been guided by a theoretical perspective that we summarize in 10 basic principles.

1. The arms race is only one manifestation of the pervasive rivalry between the superpowers. Our analysis is limited to data on U.S. and Soviet military budgets, COPDAB events data,

and U.S. national income; but this analysis builds on a broader conceptualization that explicitly addresses the challenge of foreign policy substitutability as posed by Most and Starr (1984). Rival states need not react in kind to each specific type of behavior but can instead select from a wide range of policy instruments. As a consequence, no one policy area can legitimately be considered in isolation. Instead, any model of superpower rivalry must encompass the same breadth of behaviors as is considered relevant by the decision makers themselves.

2. Superpower rivalry is grounded in persistent patterns of domestic political competition. In other words, arms race reaction occurs not between states per se but rather through the unfolding of political competition within each state. Security policy is determined by competition among individuals with different interests and perceptions, each striving to convince others to support or accept his or her preferred policies. Despite these differences, policy opponents share the assumption that the behavior of the rival must be carefully scrutinized, because they all perceive the rival as a vitally important threat to national security (even though they may sharply differ on the magnitude of that threat at any particular time). These reciprocal perceptions of threat link the domestic political processes of the two superpowers into a single rivalry system, in the sense that neither state's behavior can be understood without simultaneous consideration of its rival.[2]
3. This system of superpower rivalry has demonstrated a remarkable stability despite many political and technical changes, and domestic political competition plays a major role in sustaining this systemic stability.[3] Consider what happens after an improvement in superpower relations. Those actors most suspicious of the rival's real intentions would push for continued vigilance. Conversely, when harsh rhetoric, crises, or an accelerating arms race poison superpower relations, those influential actors most concerned about the dangers of nuclear war or the excessive costs of the arms race would exert pressure for an improvement in relations. (Just such pressures were felt by President Reagan even though widespread suspicions of détente contributed to his election.) Through our analysis we hope to uncover important aspects of the self-regulating dynamics sustaining the regularized patterns of interaction between these rival states.
4. We focus our analysis on the total yearly military budget, which we interpret as a summary measure of the collective outcome of a wide range of (interrelated) debates over specific weapons systems and policies. Debates over military procurement and other aspects of security policy are guided by information on the rival's behavior and on the overall state of the economy. Thus, these latter factors are included as information variables in our analysis of spending levels. Similar analysis could be applied to other policy variables, but this distinction between information available to policy makers and the policy instruments they manipulate must be kept in mind.
5. One can hardly read the record of security policy debates without developing a deep appreciation for the myriad ways in which information from a dazzling range of sources is used by various actors. Office holders, political activists, civilian and military bureaucrats and strategists, consultants, and other participants in

security policy debates actively seek out information about the rival and base their proposals on their expectations of future events. Different policy actors routinely emphasize different types of information, with supporters of increased military spending stressing perceived security threats while opponents generally counter with concerns about possible economic costs. Frankly, the efforts of policy makers to anticipate future threats is difficult to reconcile with the passive reaction posited in many previous analyses of the arms race. Consider, for example, the "reactive linkage" model in Ostrom 1978, in which military budgets are determined by the standard operating procedures of a small number of bureaucratic organizations acting in a well-defined sequence (see also Majeski 1983 and Ostrom and Marra 1986). Our approach more closely resembles "expectational linkage," since the dynamic policy processes of rival states are linked together through the expectations of participants in policy debates.

6. Policy actors within each state use whatever information they feel is relevant for gauging the capabilities and intentions of their rival. Each state's security policies emerge from competition among actors with different expectations, each using information to influence each other's expectations; that is, superpower competition is driven by a process of *sophisticated reaction* to any information relevant to an assessment of the levels of future threat the rival is likely to pose, not by direct reaction to any single set of figures or any particular source of information. We consider our approach to be considerably less restrictive than Ostrom and Marra's (1986), for whom actors respond only to the CIA estimate of Soviet spending (lagged three years) plus a small number of other variables. Also, in previous "adaptive expectations" arms race models actors can only extrapolate past expenditures (Majeski 1984, 1985; Majeski and Jones 1981). We allow the decision makers themselves to decide what information is relevant to their deliberations.
7. Although superpower rivalry has been shaped by misperceptions and mistaken beliefs, it is also reality-based in the sense that the expectations of the rival's future behavior that play such crucial roles in policy debates are sensitive to observed changes in the policies and actions of both superpowers, as well as other sources of technological, political, and economic change. Individuals with different preferences may well interpret the same information differently, but any individual's biases will be taken into account by his or her colleagues. Some individuals may use biased estimates of rival behavior or future economic conditions to strengthen their case for specific policy proposals, but the intensely competitive nature of policy debates ensures that no biases will remain unchallenged. This competitive and sophisticated use of information motivates our basic—and controversial—contention that so much information is used effectively by so many individuals as to approximate the phenomenon of rational expectations studied by macroeconomists.[4] We argue that the security policies of rival states approximate the behavior of two unitary rational actors informed by rational (unbiased) expectations of each other's behavior.
8. Any expectational approach to decision making focuses attention on unexpected change, rather than change per se. Any surprising and unexpected event constitutes an innova-

tion in the information available to decision makers. Observation of important innovations should lead actors to change their expectations and their behavior, whereas observations of expected policy changes need not lead to any adjustments. Thus, any dynamic model must control for the effects of expected changes. As a consequence, our statistical analysis will be directed not so much at parameter estimates as at the interrelationships among error terms in different equations, since each error term denotes the remaining unexplained variance in that variable.

9. Perhaps the most dramatic contrast with previous analyses is our reluctance to impose any single model of the arms race. Precisely because of the importance of information and expectations, the dynamic processes of policy competition are so complex and changeable they preclude the imposition of any fixed model of routinized behavior. Rather than arbitrarily selecting any single structural model from among the wide array of possible models, we choose instead to be guided by the historical record of superpower behavior as reflected in time series data on aggregate indicators of their behavior. In particular, we use vector autoregression models that enable estimation with minimal a priori restrictions.
10. Finally, the superpower rivalry system is fundamentally bipolar in the sense that most policy actors in each superpower have consistently viewed the other as the primary threat to its own security. Our focus on information, expectations, and the distinction between policy and information variables suggests a novel interpretation of the extent of bipolarity in the current era. Although the behavior of China and other states provide important pieces of information relevant to the deliberations of U.S. and Soviet policy makers, the behavior of these states is primarily determined by factors exogenous to the rivalry system per se. Also, once the superpowers have adjusted to the new reality of triangular politics, an independent French nuclear force, or the economic clout of OPEC, the fundamental importance of their underlying bilateral rivalry remains.[5] In any event, we model the dyadic system of superpower rivalry, not the world system as a whole.

To recapitulate, we interpret the superpower arms race as a manifestation of a stable system of pervasive rivalry sustained by a process of sophisticated reaction of domestic political competitors using a wide range of information on expected future events to exert influence over policy. Some of our empirical analysis may superficially resemble tests of simple arms race models, but these 10 principles establish a very different context for the interpretation of statistical tests. The crucial point is that although we as analysts have chosen to restrict our attention to a small number of aggregate level indicators of policy and information, the decision makers themselves must observe a much wider range of variables at all levels of aggregation. For if rival states merely react to the size of each other's military budget, our sophisticated reaction model would be dead wrong.

A Model of Sophisticated Reaction

We briefly sketch a model of sophisticated reaction in U.S.-Soviet rivalry. We develop this model more fully elsewhere (McGinnis and Williams 1989; Williams and McGinnis 1988). This presentation is intended solely to motivate our empirical tests of propositions consistent with a wide range of particular formalizations based on the same general principles.

Let i denote each individual participant in security policy debates within each state k, for $k = 1,2$. At time t each individual has access to the information set Z_{ikt}, and each actor uses this information to formulate an expected strength of the future threat from the rival, denoted by the threat assessment term T_{ikt}. Individuals also formulate expectations about the future opportunity costs (or fatigue) of their own state's security policy, which we denote by C_{ikt}. Since individual expectations will, in general, differ, let C_{kt} and T_{kt} represent some average measure of the fatigue and threat assessments of the actors within state k.

Each state's security policy emerges from the efforts of officeholders and political activists to maintain national security while simultaneously pursuing individual and sectarian agendas. From Arrow (1963) and the extensive social choice literature we know that policy choice cannot be defined as a function of individual preferences alone, since outcomes are necessarily affected by the advantages conveyed to certain actors by the structure of policy-making processes (Riker 1982). However, for our purposes these structural factors are encompassed by the information set Z_{kt}, which includes all information available to any of the actors in state k. Without further specifying a specific model of preference aggregation we can nonetheless consider some aggregate-level consequences of this expectational formulation. State k's military expenditures x_{kt} can be written as some function of the aggregate measures of the expectations of the individual participants influential in the security policy process: $x_{kt} = f(C_{kt}, T_{kt}, Z_{kt})$ for $k = 1,2$.

Although individual activists will be guided by different beliefs and interests, we presume that most individuals effective in policy debates are responsive to the input of new information, in the sense that their positions on specific policy proposals are subject to change even if their underlying beliefs and interests remain stable.[6] Formally, $C_{kt} = C_{kt-1} + \xi^C_{kt}$, and $T_{kt} = T_{kt-1} + \xi^T_{kt}$, where ξ^C_{kt} and ξ^T_{kt} denote the innovations in the aggregate-level fatigue and threat assessment terms. This implies the following dynamic representation of each state's expenditures: $x_{kt} = f(C_{kt-1} + \xi^C_{kt}, T_{kt-1} + \xi^T_{kt}, Z_{kt})$, for $k = 1,2$; that is, each state's expenditures should be a function of the previously expected values of fatigue and threat, as adjusted by new information. If the rivalry between the superpowers is pervasive enough to cover a wide range of issues and sufficiently enduring to enable actors to draw on relevant past experience effectively and if competition between actors with different preferences is so intense that it provides sufficient incentives for actors to seek information from various sources, virtually the same information will be included in both information sets Z_{1t} and Z_{2t}. If, in addition, the relevant actors are fairly sophisticated in the efficient use of information supporting their cause, these aggregate-level expectations should, in the long run, be unbiased. Otherwise, any systematic bias would be uncovered by the actors whose interests would be served by doing so.

Let $E_{t-1}(y)$ denote the forecast of the value that variable y will take on at time t, with this forecast formed according to all information available at time $t - 1$. If, in the aggregate, forecasts of military expenditures are unbiased, the following relationship holds:

$$\begin{aligned} & x_{kt} = E_{t-1}(x_{kt}) + \xi^x_{kt}, \\ & \quad \text{with } E_{t-1}(\xi^x_{kt}) = 0, \\ & \quad \text{for } k = 1,2. \end{aligned} \tag{1}$$

In words, one year's expenditures are a function of the expected expenditure level based on the information previously available, plus whatever innovations occur in expenditures, denoted by ξ^x_{kt}. The first part of equation 1 holds by the definition of an innovation as unexpected change, but the second part requires these forecasts to be unbiased. Technically, under rational expectations, innovations have zero mean and are uncorrelated with the associated expectational value as determined at the previous time period.

This formal argument can be summarized in our *rational expectations hypothesis:* under sophisticated reaction, the military budgets of rival states should approximate the optimal allocations of two unitary rational actors capable of forming unbiased forecasts of each other's behavior. Although we realize that states are complex conglomerations of politically motivated actors pursuing a variety of policy interests, we argue that intense and pervasive competition to influence policy will induce actors to gather and make effective use of such a wide range of information as to approximate efficient competition over policy.[7]

Implications of Sophisticated Reaction

We test some implications of this rational expectations hypothesis for the direct relationship between the military expenditures of the United States and the Soviet Union elsewhere (Williams and McGinnis 1988). Here we examine some of the aggregate informational linkages between the superpowers' security policies. Specifically, we evaluate the sophisticated reaction model in terms of diplomatic hostility and economic conditions, which should be important elements of the information sets of both states. In lieu of postulating any restrictive structural model we instead use vector autoregression methods to analyze the statistical properties of a system of variables selected to represent important policy and information components of superpower rivalry.[8]

Let the two states' military expenditures in year t be denoted by x_t and y_t, and relevant information variables by z_{kt} for $k = 1, \ldots, K$. Then the following vector autoregression system of $K + 2$ equations needs to be estimated.

$$\begin{aligned}
x_t &= \sum_{i=1}^{N} a_i x_{t-i} \\
&\quad + \sum_{i=1}^{N} b_i y_{t-i} \\
&\quad + \sum_{j=1}^{K} \sum_{i=1}^{N} g_{ji} z_{jt-i} + u_t \\
y_t &= \sum_{i=1}^{N} c_i x_{t-i} \\
&\quad + \sum_{i=1}^{N} d_i y_{t-i} \\
&\quad + \sum_{j=1}^{K} \sum_{i=1}^{N} h_{ji} z_{jt-i} + v_t \\
z_{kt} &= \sum_{i=1}^{N} p_{ki} x_{t-i} \\
&\quad + \sum_{i=1}^{N} q_{ki} y_{t-i} \\
&\quad + \sum_{j=1}^{K} \sum_{i=1}^{N} r_{kji} z_{jt-i} + w_{kt} \qquad (2)
\end{aligned}$$

for $k = 1,2, \ldots K$.

Previous distributed lag specifications of two military expenditure series, as in Majeski and Jones 1981 and Williams and McGinnis 1988, amount to a special case of this formulation (for $k = 0$). Nonexpectational arms race models that include other explanatory factors such as hostility or gross national product (GNP) can also be considered special cases, but the standard interpretation of parameter estimates as a measure of the strength of reaction is inappropriate. Since this set of equations represents a reduced form, each parameter includes the effects of feedback and actor expectations.

In Williams and McGinnis 1988 we analyze data on military budgets. Under sophisticated reaction a state's military procurement and overall budgetary outlays emerge from comprehensive debates over the capabilities and likely intentions of the rival. Unless new information is observed there is no reason to change policy. Hence, expected changes in the rival's expenditure levels should not induce changes in a state's behavior, whereas unexpected deviations may indeed do so. If forecasts are unbiased, or rational, neither of the states' past military expenditures will predict the rival's.[9]

Even if forecasts are unbiased, international relations provides such a complex and changeable decision environment that surprising events occur very frequently. When surprising new information is observed, the expectations of individual actors and the overall policy may change. Furthermore, both states should respond similarly to unexpected developments, because information relevant for one state will be relevant for the other. Actors in each state should anticipate the changes in the rival's behavior that will result from any unexpected event and will use these revised expectations to effect changes in their own state's policy. Therefore, observed deviations from expected levels of military spending (policy innovations—represented by the error terms in the estimated equations) should be contemporaneously and positively correlated.

For example, evidence that the rival is engaged in research to develop some new weapon system will strengthen support for an increase in one's own research effort. Since this policy change could not have been predicted on the basis of previously available information, it represents a policy innovation. Given the importance of superpower rivalry and the intensity of domestic debates, actors within one state will hardly wait to see exactly which weapons the rival develops before beginning their own research project. Innovations in any part of the rivalry system will elicit responses by political agents in each state, thereby linking the states' policies through sophisticated reaction.

Briefly, empirical analysis strongly supports both predictions: the two states' military expenditures are statistically independent, and their innovations are contemporaneously correlated at $r = .56$. If these results truly reflect the operation of a pervasive rivalry system (rather than reflecting some idiosyncracies of military expenditure data), innovations in the information variables of diplomatic hostility and economic conditions should be correlated with innovations in defense spending. This follows because this particular information is so crucial to policy debates on military spending.

Testing the information to policy linkage raises additional complications. If actors use information on variable Z to shape policy variable Y, it may seem intuitively plausible that the information variable Z should help predict Y (in a Granger causal sense). However, observation of a lack of causality between Z and Y could be interpreted as evidence that (1) this information is not used by the actors deciding on that policy, or (2) this information is used very efficiently indeed. The contemporaneous correlation between innovations is so crucial to our analysis because such correlations have an important interpretation within a rational expectations framework.

To further clarify this information to policy linkage, consider the consequences of some surprising event relevant to predictions of either state's hostility or economic conditions. If this new information is quickly incorporated into the revised expectations of relevant actors, and if policy is quickly adjusted to conform with these new expectations smoothly and efficiently, rational expectations will be most closely approximated. Innovations in information and policy will be contemporaneously correlated, and all relevant past information will be summarized in the previous period's defense expenditure. Thus, all lagged data other than lagged expenditures will fail to predict future spending. Realistically, however, it takes time for actors to revise their expectations, and additional delays in policy implementation may be caused by bureaucratic inertia or the lead time needed to develop new weapons systems. Such "inefficiencies" in this abstract transformation of information innovations into policy innovations constitute deviations from rational expectations and will produce empirical observation of across-time interdependence of the series as well as attenuated contemporaneous correlations of innovations.[10]

In summary, the following empirical regularities are predicted by the sophisticated reaction model as long as inefficiencies in the transformation from information to policy are minimal. First, contemporaneous correlations among innovations in information and policy variables should be relatively large, as political agents respond to unexpected changes in the international and domestic environments. Second, contemporaneous across-variable effects should overwhelm lagged effects, as political agents act on future expectations. Third, the response of policy to information innovations should reflect the logic of rivalry, in the sense that a positive innovation in hostility of the Soviets toward the United States should result in positive increases in U.S. military spending, and so forth.

One consequence of our model deserves special attention. Despite the broad range of variables included in standard arms race models (Isard and Anderton 1985), in no case have one state's economic conditions been included in an equation for the other state's military expenditures. Yet the sophisticated reaction model implies that one state's economic conditions convey information about that state's ability to arm and that this information should affect the rival's expectations and policy.

Furthermore, sophisticated reaction implies that not all variables will be linked in the same way. Information on the rival's level of hostility is certainly useful in policy debates, but a state's own hostility is less a source of information than a second arena of policy debate. In general, the values of these two policy instruments should not be expected to move together in any consistent fashion. After all, their values are set by different individuals or organizations within the government: a single presidential speech can dramatically alter the tenor of diplomatic relations, but changes in military budgets require complex and time-consuming compromises with Congress. If the actions of different government bureaucracies were highly coordinated, a large positive contemporaneous correlation would be expected, indicating a strong complementary relationship between hostility and spending. Conversely, a large negative correlation would require a consistent substitutability, in which any weakness in one policy instrument is systematically compensated for by strength in the other. Since hostility and spending can operate as complements or substitutes, their relationship is necessarily very complex. Furthermore, even though some exogenous shocks should have common effects on both policy areas, expenditures should be subject to additional influences less directly relevant to diplomatic behavior, and vice versa.

All this implies a basically ambiguous relationship, in that small correlations could exist even if a state's diplomatic behavior is closely related to its spending decisions. In conclusion, ambiguous empirical relationships between a state's own expenditures and hostility are likely, but we are confident that across-nation interactions will closely correspond to the implications of the rational expectations hypothesis.

Analysis of Unrestricted Vector Autoregression Models

The selection of a data series for Soviet military expenditures is necessarily controversial, but two widely used data series are those provided by Stockholm International Peace Research Institute (SIPRI) and the CIA. By applying a battery of ARIMA models to CIA data (U.S. Congress, Joint Economic Committee 1982a, 279–81), we found that it most closely fit an ARIMA(0,1,1) model, that is, one with a large moving average component. The relevant estimates are as follows:

$$Y_t - Y_{t-1} = \underset{(.47)}{1.65} + \underset{(.17)}{.47}\, e_{t-1} + e_t,$$

$$Q = 7.02,\ p = .93$$

Remarkably, the data fit almost exactly what one would expect if CIA counting of Soviet defense activities overlapped by two years; that is, approximately one-half of the error in the last period's spending estimate innovations are responsible for the current period's spending innovations. Thus, CIA procedures apparently smooth out yearly variations in order to stress long-term trends in Soviet military outlays.[11] Therefore, we use the SIPRI series because it has proven more sensitive to the yearly changes in budgetary levels most directly relevant to the predictions of our model.[12]

Perceptions of improving or worsening diplomatic relations play important roles in debates over security policy, and events data provide useful indicators of the overall state of relations. Since we need an aggregate measure of diplomatic relations that is sensitive to changes in the intensity of relations, we use the COPDAB data set (Azar 1980) with events scaled as in Azar and Havener 1976.

We compared two alternative uses of the COPDAB data. Most previous research uses a "net" hostility score, defined as the sum of conflictual acts minus the sum of cooperative acts, with each act weighted according to the Azar-Havener scale. A net hostility score has strong face validity as a measure of the overall state of diplomatic relations, despite some evidence that cooperation and conflict may comprise separable dimensions of interaction (Frei and Ruloff 1988; Ward 1982). However, a net measure makes little sense under sophisticated reaction. Domestic actors use information to try to influence other actors, and they would probably use information concerning the conflictual and cooperative acts of the rival state in different ways. For example, participants in U.S. policy debates make frequent and repeated use of instances of aggressive Soviet behavior, but opponents of increased military expenditures virtually never counter with instances of "good" Soviet behavior. Instead, opponents typically focus on the potentially detrimental consequences on the U.S. economy or on other policy goals. Although a rival's cooperative actions would not be ignored, they should have a more problematic connection to observed military expenditure levels, since they would be less useful—and have a different meaning—in domestic policy debates. Therefore, an alternative "pure" hostility measure consisting of the weighted sum of the conflictual acts alone seems more appropriate for the sophisticated reaction interpretation than a "net" measure.

To select between these alternative measures we estimated two four-variable vector autoregression (VAR) models using the net and pure hostility scores (along with the two military expenditure series for the years 1949–78). The contemporaneous correlations of innovations are

Table 1. Contemporaneous Correlations of Innovations for Unrestricted VAR Models of Military Expenditures and Hostility, 1949–78

Variable	U.S. Spending	Soviet Spending	U.S. Hostility	Soviet Hostility
Pure hostility				
U.S. spending	1	—	—	—
Soviet spending	.40	1	—	—
U.S. hostility	.28	.43	1	—
Soviet hostility	.44	.48	.55	1
Net hostility				
U.S. spending	1	—	—	—
Soviet spending	.33	1	—	—
U.S. hostility	.12	.00	1	—
Soviet hostility	.40	.17	.42	1

Note: These innovations are from unrestricted vector autoregression models including three lags of each variable.

presented in Table 1. Clearly, defense spending innovations are more closely correlated with innovations in pure hostility. This result indicates that the pure hostility indicator is more closely associated with the system of military expenditures than are the net hostility indicators. In our subsequent analysis only the pure hostility scores were used.[13]

By our argument, data on economic conditions should also be closely related to expenditure levels. The best overall summary measure of economic conditions is a state's real national income (GNP adjusted for inflation). Although GNP is by no means the only piece of information that policy makers find useful in their debates over security policies, it does provide the best summary measure of how well the economy is performing as a whole and thus how effective economic concerns are in slowing the arms race.

Ideally, we would use measures of the national income of both states, but for several reasons we report results including only a measure of U.S. GNP. First, measures of Soviet national income are controversial and considerably less reliable than U.S. data.[14] Second, economic conditions have different effects on domestic debates in these two states. Public opinion in the United States is strongly affected by economic conditions, and frequent elections create an incentive for U.S. policy makers to respond quickly to changes in public opinion. As a consequence, U.S. decision makers face a stronger defense-welfare trade-off; that is, if we could include only one state's economic conditions, we would prefer to include that of the United States. Finally, we are indeed confronted with essentially that very problem, since a five-variable model considerably increases the degrees of freedom available for estimation over a six-variable model, and we are already operating close to the edge of valid estimation.

We estimated a five-variable VAR model using data series for the years 1949–78 on U.S. and Soviet military expenditures and pure hostility and U.S. real GNP. Degrees-of-freedom restrictions prevent us from including more than three lags of each variable. A likelihood ratio test was performed to determine whether or not the addition of the two hostility variables and U.S. real national income helps predict U.S. and Soviet military expenditures or define their innovations.[15] This test accounts for differences in the off-diagonal as well as diagonal entries in the residual covariance matrices of the restricted and unrestricted models, so the test is sensitive to changes in the covariance of the disturbances as well as to differences between the diagonal elements of the error covariance matrix. This means that even though these additional variables may not significantly reduce the size of the error in the spending equations, they may yet redefine innovations and their correlations in a significant way, implying rejection of the null hypothesis. This test produced a chi-squared value of 49.7 (24), with a corresponding p-value less than .002.[16] Therefore, we can conclude that these variables belong in a VAR model including military spending.

A Bayesian Vector Autoregression Model

When the data series are so short, it is generally not feasible to include many additional variables or long lag lengths in a dynamic model. Estimating a set of unrestricted difference equations can result in overfitting sample anomalies, and economists and political scientists by convention deal with this danger by parsimonious parameterization, that is, by restricting many coefficients to equal zero. Conventional analysis presumes that a parsimonious specification of a statistical model can be based on solid, prior knowledge. However, such explicit specification would be inconsistent with our theoretical argument. Because a rational expectation is based on relevant past information, any variable important in determining one state's military expenditures will be important to the rival in the formation of expectations about expenditures. In addition to rendering parsimonious parameterization procedures questionable, using rational expectations also makes identification very difficult. In fact, "pure" rational expectations models may not be identifiable (Sims 1980).[17]

We use Bayesian methods to attain parsimonious parameterization of the vector autoregression. In order to overcome overparameterization problems associated with estimation of the large number of parameters implied in equation system 2, we chose a method of restriction based not on a priori theoretical concerns but rather on the general behavior of time series. Specifically, the Bayesian approach to the estimation of VAR models introduced by Doan, Litterman, and Sims (1984; Litterman 1984; Sims 1982) allows restriction of the parameter space without requiring the analyst to presume the truth of imprecise prior restrictions.

The Bayesian VAR (or BVAR) approach to parameter restrictions proceeds as follows. Parameter prior means are set at either one or zero, with one used for the mean of the first lag of the left-hand-side variable of each equation and all other coefficient means set to zero. In effect, this creates a random walk prior. The idea behind using a random walk is that many variables without trend or logged values of variables with trend have a naive forecast of no change. The key decisions about prior specification concern the second moments of the prior, and the tightness of the distribution around the prior mean depends on several factors. A variable's own lags get the looser priors, with other lags receiving tighter priors around their zero means, since autoregression models perform well in predicting future values of a variable. Looser priors are imposed on more recent lagged terms, with this difference based on the idea that lags with closer proximity to the left-hand-side variable should be more important than lags of variables that are further removed in time.

The specification of this prior information need not, and should not, be too

Table 2. Contemporaneous Correlations of Innovations for VAR Models of Military Expenditures, Hostility, and U.S. GNP, 1949–78

Variable	U.S. Spending	Soviet Spending	U.S. Hostility	Soviet Hostility	U.S. GNP
VAR Model					
U.S. spending	1	—	—	—	—
Soviet spending	.71	1	—	—	—
U.S. hostility	.35	.37	1	—	—
Soviet hostility	.67	.36	.68	1	—
U.S. GNP	.31	.62	.76	.48	1
Bayesian VAR model					
U.S. spending	1	—	—	—	—
Soviet spending	.45	1	—	—	—
U.S. hostility	.18	.30	1	—	—
Soviet hostility	.29	.08	.54	1	—
U.S. GNP	.24	.26	.34	.22	1

Note: These innovations are from an unrestricted and Bayesian vector autoregression including three lags of each variable.

detailed; and three Bayesian hyperparameters reduce the parameter space considerably. We placed a prior standard deviation about the lagged dependent variables of .2; other variables in each equation get one-half this weighting, scaled to the left-hand-side variable. A decay of k^{-1} (where k is the lag number) is used, which tightens the prior distribution as k increases.[18]

Before proceeding, it is important to emphasize that this use of a Bayesian prior is a neutral way of introducing restrictions on the estimated coefficients. Conversely, most methods of restricting structural equations are directly based on theoretical considerations, thus effectively biasing the empirical results in favor of a particular theoretical interpretation. The Bayesian VAR approach does not bias results in favor of finding across-equation dependencies; instead, it places the burden on the researcher to identify relationships among variables strong enough to overcome the prior.

Although we estimated models with and without the prior specification, we present detailed analyses of the Bayesian model alone, as these results are more likely to draw attention to important across-variable effects. But first we briefly examine the contemporaneous correlations of disturbances from the unrestricted VAR for evidence of linkage across states (Table 2). For the unrestricted VAR, the correlations of innovations are large, indicating that innovations in these variables all move together in a consistent way. However, attempts to interpret the relative magnitude of entries in any correlation matrix can be misleading, a problem compounded in this case by the fact that these correlations are very large and are estimated from a model that is severely overparameterized.

We present these non-Bayesian correlations primarily to alleviate any suspicions that we chose the Bayesian approach to obtain results more favorable to our interpretation. Instead, as shown in Table 2, correlations of the innovations from the BVAR are actually smaller than those of the unrestricted VAR. Nonetheless, the patterns of the correlations still indicate that hostility and military spending innovations move together. In particular, moderate correlations are evident between each state's hostility and the other's expenditures.

Dynamic Response Analysis

To further analyze the overall dynamics of this system, we next examine the transformation of a VAR model known as its *moving average representation* (MAR).[19] A typical shock (one standard deviation in magnitude) is applied to one variable and the responses of the other variables in the system to this shock are examined. If the responses of these other variables make no sense substantively or if there is evidence of strong across-time effects on defense expenditures, our confidence in the validity of the overall theoretical model would be shaken. Therefore, this type of analysis takes on considerable importance in evaluation of our rivalry model.

Estimation of equation system 2 demonstrated that these innovations are contemporaneously correlated. In order to analyze the dynamic response of the system to an exogenous shock it is necessary to impose some ordering on the direction of influence implied by the contemporaneous covariances. Furthermore, this ordering of contemporaneous relations must be consistent with some hypothetical causal structure grounded in some particular theoretical interpretation; that is, an ordering selected at random need not result in a realistic representation of the dynamic properties of the system.

Econometricians generally use simple causal chains to provide a structure for these contemporaneous relationships (Sims 1980). Choleski decomposition is used to transform the covariance matrix Σ into the lower triangular matrix S where $SS' = \Sigma$. This procedure is appropriate for two major reasons. First, many contemporaneous correlations in economic and political models are not large and are therefore of secondary importance in analysis. Second, the moving average projections are robust to changes in the ordering of causal influences when these correlations are small.

For our analysis of the dynamics of U.S.-Soviet rivalry the ordering of the contemporaneous relationships becomes both more consequential and more problematic. The presence of large contemporaneous correlations implies that different orderings can produce significantly different dynamics. Strategic interaction between states and the general norm of reciprocity in diplomatic behavior leads us to expect that many important across-equation effects are truly contemporaneous. For these reasons we would prefer to maintain some simultaneous response rather than impose a complete triangular causal chain. Since we are not able to maintain simultaneous orderings for both hostility and expenditures, we choose to specify enough zero-order restrictions on the contemporaneous structure to identify a "quasi-structural" simultaneous model of military expenditures.[20] Briefly, we identify the simultaneity between military expenditures, and not diplomatic hostility, because we treat the former as our primary measure of security policy.

A quasi-structural model represents relationships among innovations in our five variables, with these innovations defined as the residuals in the estimated Bayesian VAR model of the overall structure of superpower rivalry. This contemporaneous structure essentially imposes an ordering on the rapidity with which the effects of innovations in one variable are transmitted to the others; that is, when variable A is ordered ahead of variable B this means that innovations in A immediately induce innovations in B whereas innovations in B affect A only after some lag. But this restriction in no way implies the lack of a mutual relationship between A and B.

Many contemporaneous orderings can be posited for any single estimated VAR model, but not all orderings would be equally plausible. We justify our choice of restrictions as follows. First, diplomatic hostility provides information on the international environment of security policy making, and so both hostility variables are placed before the spending terms in the ordering. Second, each state's hostility innovations are included in the equation for the other state's spending innovations, but not its own. As discussed earlier, the rival's diplomatic behavior is frequently used in budgetary debates, but the linkage between a state's diplomatic behavior and its own expenditures is more ambiguous.

Third, the different roles played by domestic political opinion in these two states justifies an asymmetry in the hostility terms. A surprising level of Soviet hostility will generate domestic pressures for U.S. retaliation in kind, whereas Soviet leaders are less tightly constrained in their diplomatic responses to surprise U.S. moves. Thus, we impose the restriction that Soviet hostility precedes U.S. hostility, since (relatively speaking) the U.S. responds more rapidly to the immediate effect of Soviet diplomatic messages than vice versa.

Fourth, U.S. GNP is affected primarily by factors exogenous to our model, and thus it occurs first in the ordering. Fifth, and finally, GNP innovations enter into all of the other equations, with one exception. We are able to include GNP in the equations for both hostility innovations because they create a triangular block, and we allow U.S. GNP to affect U.S. spending immediately, but GNP is restricted to affect Soviet spending only through lags. Again, the primary rationale for this distinction lies in differences between the rapidity with which the effects of innovations are transmitted to other variables; it does not imply the absence of a relationship between U.S. economic conditions and Soviet military expenditures.

Other contemporaneous structures may be equally plausible, but these restrictions enable us to integrate important aspects of the relevant political structure (especially the asymmetric effects of public opinion) with our simple and powerful rational expectations hypothesis. Ideally, we would prefer to test a less-restrictive version including more simultaneous relationships, but such an approach would require more variables and an extension of the data series. Instead, we more fully identify the interaction of the states' defense spending at the expense of modeling the interaction of their diplomatic hostility. This gives us more confidence in the robustness of projections of military expenditures to shocks in other variables than to the projections of hostility to domestic shocks and military spending.

Using *e* and *h* subscripts to denote the military *expenditures* and *hostility* of the United States (US) or the Soviet Union (SOV), these restrictions imply the following contemporaneous structure for innovations in our five variables.[21]

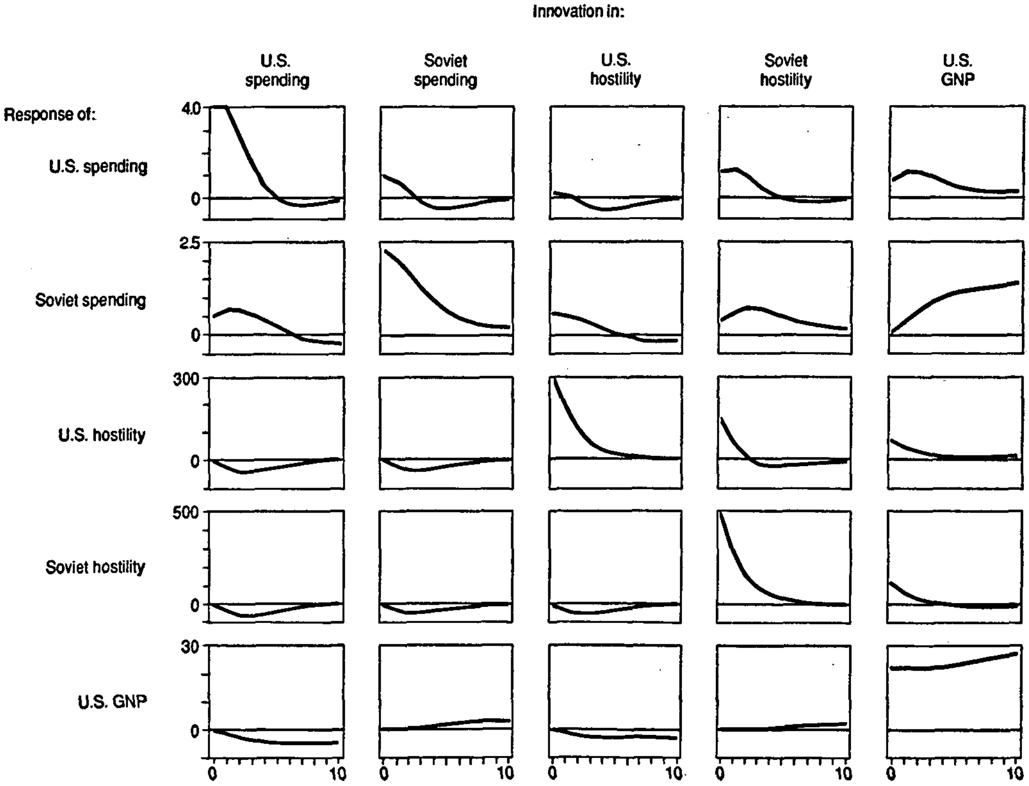
$$\begin{aligned}
US_e &= .37\ SOV_e + 2.03\ SOV_h + 24.4\ GNP + e_1 \\
SOV_e &= .13\ US_e + 1.96\ US_h + e_2 \\
US_h &= .29\ SOV_h + 3.11\ GNP + e_3 \\
SOV_h &= 4.80\ GNP + e_4 \\
GNP &= e_5
\end{aligned}$$

The dynamic response of the Bayesian VAR is illustrated in Figure 1, which includes 25 charts, each representing the response of the row variable over a 10-year horizon to a one-standard deviation (typical) innovation in the column variable. Not all of these response charts are equally consequential. A quantitative indication of their relative importance is given in Table 3. Each variable's forecast error can be decomposed into the percentages due to innovations in each of the variables. For example, errors in forecasts for next year's U.S. military spending result not only from innovations in U.S. spending per se but also from innovations in each of the other variables. The numbers across each row correspond to the percentage of forecast error (*k* years ahead) in that row variable that is accounted for by the variable listed at the top of the columns. For example, 6.4% of the error in the one-year-ahead forecast of U.S. spending is due to unexpected changes in Soviet hostility. Each variable usually accounts for a large amount of its own variation; but if across-variable effects are present, the percentage of variance explained by innovations in other variables should be relatively large.

Figure 1 is most easily interpreted by groups:

1. The self-response charts present the effects of an innovation in one variable on itself. Each of the defense spending

Figure 1. System Response to Innovations



Note: Each chart represents the response over 10 years of the row variable to a one-standard-deviation positive shock (innovation) in the column variable. The vertical scales are in the following units: military spending in billions of constant 1970 dollars, hostility in units of the Azar-Havener COPDAB intensity scale, and U.S. GNP in billions of constant 1972 dollars.

variables decays, returning to preinnovation levels relatively quickly. Similarly, hostility variables also return to their preinnovation levels and do so even more quickly than spending. The hostility variables exhibit nearly identical decays, whereas U.S. military expenditures decay more quickly than Soviet expenditures.

2. The basic logic of arms race reaction and fatigue is clearly apparent in the reaction of one state's expenditures to an innovation in rival spending: positive innovations in either state's expenditures lead to a positive response by the other, soon followed by reduction in the spending levels of both states. Again, U.S. spending decays more rapidly than Soviet spending.
3. The most interesting effects concern how one state's expenditures respond to innovations in rival hostility, as this relationship lies at the heart of our postulated information to policy linkage. In both cases the effects are initially positive, clearly reflecting the basic arms race logic of an increase in perceived hostility leading to increases in the military expenditures of both rival states. As before, U.S. expenditures

Table 3. Decomposition of Error Variance of the Bayesian VAR: Percentage of Forecast Error Resulting from Innovations

		Innovation				
Forecast Error	*k*	U.S. Spending	Soviet Spending	U.S. Hostility	Soviet Hostility	U.S. GNP
U.S. spending	1	86.3	3.6	.2	6.4	3.5
	2	85.3	2.8	.1	6.9	4.8
	3	84.2	2.3	.2	7.1	6.2
	4	82.8	2.3	.6	7.1	7.2
	5	81.5	2.7	1.1	7.0	7.8
Soviet spending	1	4.9	86.4	5.6	2.9	.3
	2	6.5	81.7	5.3	4.9	1.5
	3	7.5	76.2	5.2	7.0	4.0
	4	7.9	71.4	4.8	8.4	7.5
	5	7.6	67.1	4.4	9.2	11.7
U.S. hostility	1	0	0	77.3	18.3	4.4
	2	.4	.3	78.8	16.0	4.6
	3	1.3	.9	78.4	14.6	4.7
	4	2.2	1.6	77.3	14.2	4.7
	5	2.8	2.0	76.3	14.2	4.7
Soviet hostility	1	0	0	0	95.4	4.5
	2	.5	.2	.2	94.7	4.3
	3	1.6	.7	.8	92.8	4.1
	4	2.6	1.2	1.3	90.8	4.0
	5	3.3	1.6	1.6	89.6	3.9
U.S. GNP	1	0	0	0	0	100.0
	2	.1	0	.1	0	99.7
	3	.6	0	.4	0	99.0
	4	1.2	0	.6	0	98.2
	5	1.7	.1	.7	0	97.4

Note: Each entry represents the percentage of forecast error (*k* years ahead) in the row variable that is due to innovations in that column variable.

decay more quickly than Soviet expenditures.

4. The remaining relationships are less interesting, and (as is reflected in Table 3) less consequential. Both hostility variables exhibit a negative (but very small) response to a positive shock in either state's expenditure terms. This may reflect some across-time substitutability, since expenditure innovations only feed back into hostility changes after one year. The asymmetrical response exhibited by the pair of hostility variables is attributable to the block triangular restrictions. Since Soviet hostility contemporaneously influences U.S. hostility, but not vice versa, U.S. hostility innovations have only marginal effects on Soviet hostility, as is demonstrated in Table 3. These results may be particularly sensitive to the particular ordering and so should not be overinterpreted. Finally, the influence of a state's hostility innovations on its own military expenditure term are complicated by the ambiguous mixture of a complementary relationship and substitutability discussed earlier.
5. Finally, several complications are entailed in evaluation of innovations in U.S. national income, since the GNP series exhibits some instability by continuing to rise after being shocked. This instability results because GNP is primarily determined by factors exogenous to this rivalry model, and other economic variables would have to be included in order to ensure stability in the GNP equation. In response to innovations in GNP, both hostility terms decay, and U.S. expenditures rise for a few years and then decay; but Soviet expenditures increase throughout the 10-year horizon. The Soviet pattern of response is in part a consequence of instability in the GNP equation, in that Soviet spending increases as GNP continues to increase.[22] Although this response pattern is quantitatively ambiguous, the qualitative pattern is interesting and suggests that Soviet spending responds to U.S. economic growth, perhaps in expectation that the capacity for a military buildup is greater as the U.S. economy expands.

Interpretation of a few of the response charts is complicated by the imposition of contemporaneous restrictions needed to evaluate the MAR. We have examined other contemporaneous structures, and each entails at least as many difficulties in interpretation. The dynamics implied by this contemporaneous structure may not be replicated under alternative representations, but this should not be taken as evidence that our results are sensitive to perturbations in the overall structure of superpower rivalry. The fundamental structure of superpower rivalry was revealed instead in the past dynamics of the system as estimated in the VAR model. With this caveat, this particular quasi-structural model successfully captures several aspects of the dynamic interactions among military expenditures, diplomatic hostility, and economic conditions that characterize superpower rivalry.

Discussion

Because of the mode of empirical analysis forced upon us by our theoretical perspective, we are unable to provide the summary tests of statistical significance so familiar to arms race researchers. Nonetheless, the estimated Bayesian VAR model provides a summary description of the statistical properties of these data series, and this description turns out to be remarkably consistent with our sophisticated reaction model. In no instance did our data analysis reveal evidence that can be interpreted as contradicting the rational expectations hypothesis.

What results could have falsified the rational expectations hypothesis? First, if incorporation of hostility and economic conditions resulted in no noticeable improvement over analyses of military expenditures alone, these information variables could hardly be as important in policy debates as our sophisticated reaction argument implies. Second, if the contemporaneous correlations between innovations for most pairs of variables had been low, there would be no evidence for any reaction between the two superpowers or for the efficient use of information in policy debates. Third, had the correlations using the more standard net measure of hostility been as large as for the pure measure, cooperation and conflict would play comparable roles in domestic debates, a result that would be very difficult to reconcile with our conceptualization of rivalry. Fourth, anomalous dynamic behavior in the MAR analysis would strongly suggest an inappropriate specification of the relationships among the variables (but perhaps only in the quasi-structural model) or even that crucial variables had been left out of our analysis. Fifth, MAR analysis revealing strong across-time or lagged effects would support an adaptive expectations model. Instead, the analysis revealed none of these difficulties.

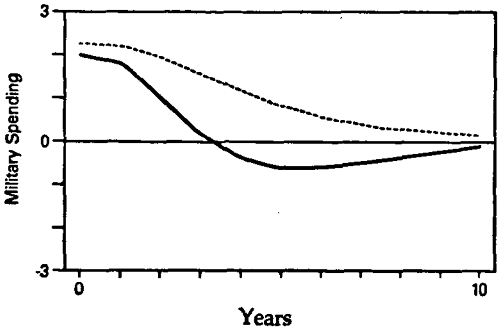
This analysis strongly indicates that the military expenditures and diplomatic hostility of the United States and the Soviet Union form a stable system of interactions (at least for the years 1949–78) and that this system is very robust to exogenous shocks. Although we have provided only indirect evaluation (at the aggregate level) of the efficiency of information use by policy competitors, the evidence indicates a remarkable degree of congruence to the rational expectations hypothesis despite the myriad sources of bias and inefficiency that seemingly plague U.S.-Soviet relations. These strong results imply that any subsequent investigation of the dynamic operation of the system of superpower rivalry should, at the very least, give serious consideration to the possible implications of rational expectations.

By no means have we exhausted the research program on superpower rivalry outlined earlier. In particular, the relationship of other important information variables (such as public opinion or the military balance) to military spending needs to be explored in more detail. Also, exploration of the full range of the rivalry system would require a fuller representation of diplomacy and other policy variables.

A secondary conclusion of this analysis merits more detailed elaboration in subsequent research. Both military expenditure terms exhibited a marked cyclical dynamic response, with the cycle for the United States considerably shorter. We postulate that this volatility is a consequence of the domestic political structure of U.S. policy making, in which gains made by populist groups can quickly dissipate as public opinion wanes and corporate and conservative interests regain their grip on policy.[23] We argue that those actors dissatisfied by improvement or deterioration in superpower relations would put forth the efforts required to reverse these trends, and the cyclical response prevalent in the MAR analyses indicate that this system of variables exhibits just such a periodicity (albeit of irregular duration).

Since the periods of U.S. and Soviet cycles are not equal, the two states' expenditures may generally be out of synchronization. If so, this could greatly complicate any analysis of arms race reaction. Suppose, for example, that both states react to an exogenous shock by increasing their expenditures. United States expenditures will begin to decay more quickly than Soviet spending, and this pattern of response would generally be interpreted as evidence that neither state is reacting to the other, since one state's expenditures are decreasing while the other's remain the same or even increase. Yet under

Figure 2. Response of Military Spending to Joint Positive Hostility Shock

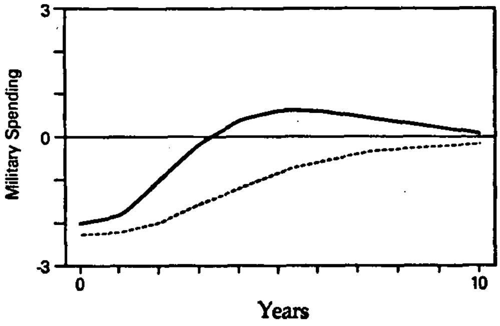


Military Spending

———— U.S. Spending
- - - - - - Soviet Spending

Note: Represents the response over 10 years of U.S. and Soviet military spending to a one-standard-deviation shock in both hostility terms. The vertical scale is billions of constant 1970 dollars.

Figure 3. Response of Military Spending to Joint Negative Hostility Shock



Military Spending

———— U.S. spending
- - - - - - Soviet spending

Note: Represents the response over 10 years of U.S. and Soviet military spending to a one-standard-deviation shock in both hostility terms. The vertical scale is billions of constant 1970 dollars.

sophisticated reaction such divergent expenditure changes are a consequence of the same exogenous change in their common environment, as mediated through domestic political systems with different characteristic decay times. If this system exhibits such complex patterns of response to a single shock, it should come as no surprise that evidence of reaction is rare in the statistical analyses reported in the arms race literature. In the current analysis, however, the basic dynamic logic of this rivalry system stands more starkly revealed.

This synchronization problem also has important substantive implications. In short, asymmetries of response can create contradictory attributions of "blame" for perpetuation of the arms race. Consider the effects on each state's military expenditures of a simultaneous shock to both states' hostility terms.[24] Figures 2 and 3 present the dynamic response of each expenditure term to positive and negative shocks in both hostility terms (each one standard deviation in magnitude). Because of the symmetry of our model, the response curves are necessarily exact mirror images of each other. Nonetheless, observation of these patterns could easily lend credence to sharply contrasting substantive interpretations.

A joint negative shock in hostility can be interpreted as negotiated "détente" or an uncoordinated response to an exogenous innovation in their environment. Whatever its source, this surprising increase in cooperation will lead some critics in both states to try to counteract what they consider a dangerous lack of preparedness. Figure 3 indicates that U.S. critics should succeed in increasing the level of U.S. expenditures while Soviet expenditures still remain low (precisely because of the asymmetric response times). However, Soviet spending will eventually increase as well. Hence, the Soviets could credibly blame this new round of the arms competition on the bad faith efforts of U.S. conservatives or the military-industrial complex.

Conversely, after a joint escalation in hostile rhetoric leads both states to increase their expenditures, other critics would work to reduce tensions. As shown in Figure 2, U.S. expenditures are likely to be the first to decrease, while Soviet military expenditures will continue at a high level. This configuration lends itself to the interpretation that the Soviets have taken advantage of unilateral restraint on the part of the United States. In this case, perpetuation of the arms race could plausibly be blamed on the intransigence of the Soviet military or the naïveté of U.S. liberals or both.

Unfortunately, these two interpretations are not entirely hypothetical. After the brief thaw in U.S.-Soviet relations after Stalin's death, U.S. hawks used fears of bomber and missile gaps to set the stage for the Kennedy administration's strategic buildup. The steady Soviet buildup of strategic forces begun after the Cuban Missile Crisis later provided a primary justification for the Reagan buildup. In the former case U.S. reaction was premature whereas in the latter case the United States was slow to counter Soviet actions. Our analysis demonstrates that these two seemingly contradictory interpretations are simply mirror images of each other. Ultimately, neither conservative critics nor Soviet intransigence can be said to "cause" the next round of the arms race, for it instead simply occurs as a natural consequence of the basic dynamics of the system itself.

This result illustrates the insidious nature of superpower rivalry: under a wide variety of exogenous shocks the dynamics of the system ensure continued rivalry. Over the past 40 years the superpower rivalry system has proven resilient to intermittent efforts to sustain higher or lower levels of animosity, and any future effort to transform the nature of superpower relations must take this stabilizing tendency into account. In the absence of a concurrent transformation of public and elite attitudes and domestic political processes in both states, the superpower rivalry system should be expected to follow its natural dynamics as revealed in this analysis.

Notes

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1. Recent reviews of the extensive arms race literature include Isard and Anderton 1985 and Russett 1983. For recent work on the related area of defense-welfare trade-offs, see Domke, Eichenberg, and Kelleher 1983; Duvall, Aldrich, and Weldes 1989; and Johnson and Wells 1986.

2. More precisely, the perceptions influential policy actors hold about the likely behavior of the rival are crucial to the continued existence of a rivalry system. For even if there were only a low probability of attack by either state, the rivalry could continue as long as most influential policy actors in each state perceived the other state as a security threat and acted accordingly.

3. In lieu of reviewing the vast literature on U.S.-Soviet relations, we refer the reader to the discussion and extensive references in Gaddis 1986 covering the many factors contributing to the surprising stability of the post-World War II international political system.

4. Muth (1960) first introduced the concept of rational expectations. For surveys of its role in the economics literature, see Begg 1982 and Sheffrin 1983; for discussion of its econometric implications, see Lucas and Sargent 1981 (intro.) and Wallis 1980; for further discussion of our specific application of rational expectations to superpower rivalry, see McGinnis 1988, Williams 1988, and Williams and McGinnis 1988.

5. We do not implement a direct test of this supporting assumption of bipolarity in this particular analysis. For related analyses that include China as a full partner in superpower relations, see Freeman and Goldstein 1989 and Rajmaira and Ward 1989.

6. Some individuals may resist the implications

of new and surprising information, as is suggested by studies of cognitive consistency and other cognitive biases (Jervis 1976). However, we argue that other actors will be more amenable to changing circumstances and that these more responsive actors will be influential in policy debates.

7. We hypothesize that states engaged in a security rivalry can be modeled as if they were unitary rational actors, not that they really are such. For further discussion of our intended connection between domestic political competition and the state as unitary rational actor assumption, see McGinnis 1988 and McGinnis and Williams 1989. For other interpretations see Achen 1988, Allison 1971, Bueno de Mesquita 1981, and Krasner 1978.

8. For more information on VAR techniques, see Litterman 1984 and Sims 1980, 1982, 1986. For other applications to political science, see Freeman, Williams, and Lin 1989 and Williams 1987.

9. When we say that a variable fails to predict another, we mean that it fails to "Granger-cause" a variable. Variable Z Granger-causes variable Y if, given the history of Y, Z improves predictions of Y. Other applications of Granger causality tests to arms races include Freeman 1983, Majeski 1984, 1985, and Majeski and Jones 1981. For a Granger analysis of events data see Freeman and Goldstein 1989.

10. If these inefficiencies are relatively similar in both states, contemporaneous correlations of innovations would still obtain. Existence of some noncontemporaneous correlations in conjunction with across-time dependence would indicate the existence of some persistent biases in information processing and use, as would be suggested by an adaptive expectations model (Majeski and Jones 1981).

11. Alternatively, if the true scores of Soviet military expenditures follow an AR(1) or random walk dynamic, the observed scores with error will follow this ARMIA(0,1,1) pattern. Chrystal and Peel (1986, 63) discuss the appropriateness of an ARIMA(0,1,1) model when a variable subject to rational expectations is measured with random error.

12. Our military expenditure data set for 1949–84 was calculated by combining data from various editions of *World Armaments and Disarmament: SIPRI Yearbook* (London: Taylor & Francis). For further details and a listing of this data, contact us or see McGinnis and Williams 1987 (app.).

13. In reporting our results we do not present the regression coefficients and their standard error estimates. The actual regression coefficients are not easily interpreted because they are not estimates of structural parameters. Instead, each coefficient has embedded within it implicit feedback, and the coefficients themselves will be choppy, in the sense that coefficients for adjacent lags may differ widely (even if the data are smooth). The moving average representation we analyze below contains the equivalent information, but in a form which is more easily interpreted, since a smooth data series implies a smooth MAR.

14. Preliminary analysis using an official series on Soviet net material product and a CIA estimate of Soviet GNP (U.S. Congress, Joint Economic Committee 1982b, 52–54) indicated that these measures are not related to our other variables. Whether this means that these particular indicators are irrelevant to the Soviet defense-welfare trade-off or whether no such trade-off exists we can not tell. Of course, the fact that U.S. intelligence agencies are concerned with such matters supports our contention that domestic actors consider as much information about the other state as possible.

15. We do not perform formal tests of causality because these tests will be adversely affected by the small degrees of freedom. In general, when the numerator and denominator degrees of freedom move closer together in value, as they will with long lag lengths and small error degrees of freedom, the F-distribution will have fat tails and tests will be biased towards rejecting the null.

16. We used the modified likelihood ratio $(T - c)$ $[\log|\Sigma_R| - \log|\Sigma_U|]$, where T is the number of cases, c the number of variables in one equation, and Σ_R and Σ_U are the estimated variance-covariance matrices for the restricted and unrestricted models respectively (Sims 1980, 17). This modification replaces T by $(T - c)$ and (though asymptotically equivalent to the unmodified test) adjusts for bias in small samples.

17. Econometric rational expectations models must rely on different types of restrictions to obtain identified equation systems. Examples include the use of incomplete information sets (Barro 1976) and cross-equation restrictions that derive from the assumption that some variables follow martingale processes (Sargent 1976). Also, more conventional methods can test the type of restrictions imposed by alternative models of expectations formation and use, as in Mishkin 1983.

18. This analysis was implemented with the Regression Analysis of Time Series statistical package, with these "specify" parameters: tight = .2; .5 weights on other variables; and decay = 1.

19. The relationship between the vector autoregression and moving average representation coefficients can be written as $A(L) = [I - B(L)]^{-1}$, where the square matrix A contains the MAR coefficients and the B matrix contains the estimated Bayesian VAR parameters.

20. The resulting model is referred to as quasi-structural because, although forced to identify some parameters, we do not interpret the parameters in the typical way. Rather, we identify the contemporaneous structure for purposes of interpreting the dynamic response patterns.

21. We use a two-stage estimation approach where we first estimate the reduced form VAR and obtain the variance-covariance matrix (Σ) of the disturbance terms from these equations. The second

stage requires estimation of a structure from this Σ matrix, which is symmetric but rarely diagonal. Technically, we fit using maximum likelihood methods the model $C = S\Omega S'$, where elements of S are the structural parameters, Ω is a diagonal matrix with elements corresponding to the behaviorally independent sources of error, and C the estimated covariance matrix of the estimated residuals from the reduced form. For further details on these quasi-structural models, see Sims 1986.

22. We did not log GNP values because we had no reason to expect a logarithmic relationship between GNP and military spending. Stability is not a problem with the remaining equations, since the response charts clearly show that variables eventually return to original levels after a shock.

23. This tentative explanation of the sources of cyclical tendencies in U.S. policy is related to the idea of "reform cycles" (Edelman 1964; Lowi 1979; McConnell 1966).

24. Since any state's hostility innovations may be quickly reciprocated by its rival, hostility innovations would tend to be simultaneous, at least for the yearly aggregates used in this study. (For analyses of the consequences of temporal aggregation in time series analysis, see Freeman 1989 and King 1989.) Thus, by applying a simultaneous joint shock, we can partially alleviate any problems of interpretation introduced by the triangular ordering of hostility terms imposed in our quasi-structural model.

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MACROPARTISANSHIP

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From an early, incorrect consensus that party identification was free of the short-term influences of political life, its aggregate, macropartisanship, drew little scholary notice. Though macropartisanship, typically seen as a biennial time series, appears essentially constant, our quarterly treatment demonstrates substantial and notably systematic movement of this crucial barometer of the U.S. party system. We demonstrate that it varies systematically with respect to time, has electoral consequences, and can be modeled as a function of economic evaluations and approval of the incumbent presidential administration. Macropartisanship, we argue, is a variable like others, subject to routine ebb and flow as citizens in the aggregate reflect their experiences of politics onto the parties. Its medium-term movements of considerable magnitude are lasting enough to matter but occur without connoting shifts in the underlying party system and can be understood without invoking the crises and convulsions of realignment theory.

Party identification is the key concept of U.S. electoral research. Always in the forefront in the analysis of individual behavior and attitudes, it is all but obvious that its aggregate, the national partisan balance, should be a central barometer of the party system. But owing to an early consensus that individual identifications did not respond to the current issues, personalities, and conflicts of politics, its aggregate was presumed to be a constant, not a variable. That early consensus, we now know, was wrong. And if individual party ties respond to issues, performance, or whatever, the partisan balance ought to vary over time. We assert that it does, that the variation is patterned, that it has electoral consequences, and that it can be explained.

Just as party identification is the key concept in studies of the individual voter, its aggregate—what we term macropartisanship—is central to theories of party system and voter alignment.[1] For macropartisanship, constancy is the norm. Change is expected only during the rare realigning transition to a new party system. And any such epochal change in macropartisanship that has occurred has gone unobserved for the reason that even the most recent supposed realignment (of the early 1930s) predates modern survey research measurement of party identification. Macropartisanship in the current era is agreed to be marked by stability. More specifically, the consensus is that changes in macropartisanship should be infrequent, small, and of brief duration. That too is wrong.

Previous Work

What is party identification? The standard view, traced to *The American Voter* (Campbell et al. 1960) is that identification is a stable psychological attachment to one's favored political party. The evidence that party identification is stable, particularly when compared to other political attitudes, appears to be quite strong. Over time, the directional component of the distribution of party identification shows a Democratic advantage of seemingly constant magnitude that varies only slightly in response to political events like landslide elections. At the individual level, changes in party identification are uncommon, at least in comparison with the turnover of responses to other political items, such as those intended to tap preferences on policy issues (Converse 1976; Converse and Markus 1979). Panel studies show that no more than about 4% of the electorate changes identification from Republican to Democratic or vice versa over a four-year period (although more will move in and out of the Independent category). Analysts have suggested that even these changes reflect measurement error more than true attitude change. (Achen 1975; Green and Palmquist 1988).

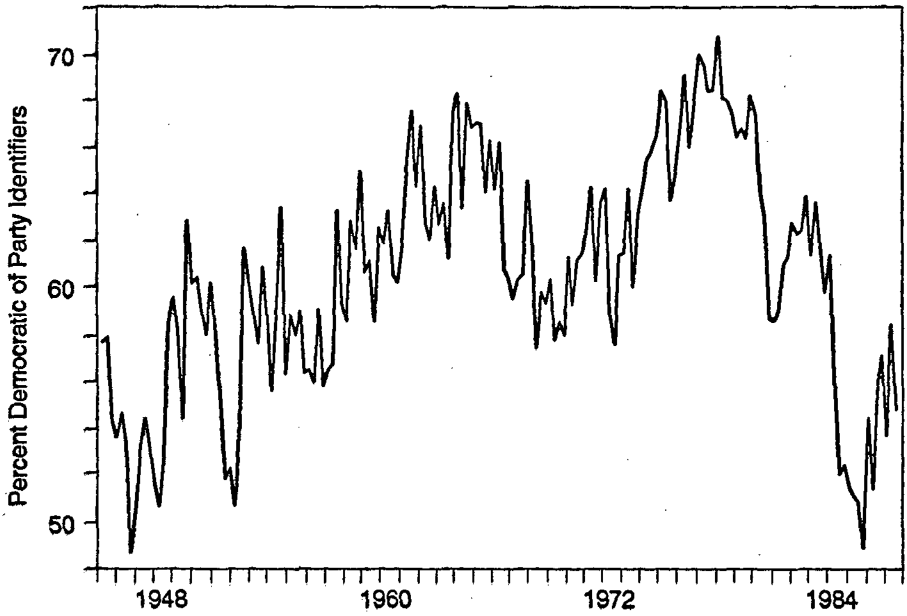
It was standard, until recently, to model the attitudinal variables affecting the vote decision to give party identification the status of the ultimate independent variable in the causal hierarchy (Declerq, Hurley, and Luttbeg 1975; Goldberg 1966; Miller et al. 1976; Schulman and Pomper 1975). Party identification was assumed to affect candidate evaluations, issue positions, and certainly the vote—but not to be affected by them. Citizens, it seemed, did not change their party preferences except during realignment events or perhaps when undergoing major changes in demographic attributes.

The reason for party identification's secure place in the voting paradigm is its stability. Voting decisions and candidate evaluations cannot cause major changes in party identification because, in the aggregate, the former variables are unstable over time while party identification is supposed not to be. Similarly, analysts have resisted the notion that issue attitudes have much influence on party identification because measures of issue attitudes are notoriously unstable while party identification is not.

Is party identification in the United States the stable psychological attachment that we have described? Over the past decade or so, party identification has been subject to some revisionary thinking (see Shively 1980 for an early history). In part, the revised view is based on growing awareness that party identification is far from perfectly stable and is indeed somewhat responsive to short-term political forces. Some evidence for revised thinking comes from simultaneous equation models of political attitudes and the vote that (with appropriate identifying assumptions) test the possibility of simultaneous effects of two variables on each other. (Erikson 1982; Franklin and Jackson 1983; Markus 1982; Markus and Converse 1979; Page and Jones 1979). These studies suggest that a major causal flow is from other variables to party identification.

Still other evidence comes from panel studies where change in party identification is seen as a function of short-term influences (Brody 1977, 1978; Fiorina 1981). The 1972–76 National Election Study provides evidence that changes in party identification were associated with perceived economic satisfaction, attitudes toward Richard Nixon, and attitudes toward Gerald Ford's pardon of Nixon (Brody 1978; Fiorina 1981). Moreover, in the 1960 wave of the earlier national panel, Catholic Democrats and Protestant Republicans tended to strengthen their identifications and Catholic Republicans and Protestant Democrats tended to do

Figure 1. Macropartisanship, 1945-88



the opposite—exactly as one would expect if people adjusted their identifications in response to the religion issue of the Kennedy-Nixon campaign (Brody 1977). Both Brody and Fiorina suggest that party identification has both a short-term and long-term component.

It is unclear how much revision in our thinking about party identification is required from such studies. It has long been known (Knoke and Hout 1974), for instance, that the aggregate distribution of party identification does change over time in response to short-term forces, but the change is thought to be slight (Campbell et al. 1960; Converse 1976; Markus 1982). Moreover, some doubt can be cast on the findings of simultaneous equation models because the models are identifiable only on the basis of assumptions that themselves are open to question. And while panels show some responsiveness of partisanship to short-term forces, it is not clear whether this responsiveness is extensive enough to be of much substantive significance.

Macropartisanship As Time Series

Some of the apparent stability of party identification is a result of how we look at it. We normally see the frequency distribution of party identification presented as a time series with two- or four-year intervals between readings.[2] Such a series looks much like the concept originally developed in *The American Voter.* Because they do not appear systematic, its year-to-year fluctuations do not draw our attention. For that we need a finer time scale.

Party identification may be treated as a continuous macro phenomenon measured through time. We have gathered data for such a series, presented here as a quarterly compilation of the Gallup identification measure from 1945 through 1987. This series is presented in Figure 1 as the

Democratic percentages of the major party identifiers. Impressionistic examination of this series suggests the presence of important systematic variation over time.

From Figure 1, Democrats can be seen to achieve "governing" majorities in the early 1960s and for most of the 1970s, with less secure, but still majority, standing at other times. But the Republicans now challenge for ascendancy, as they did once before in 1945–47. These movements in partisanship are often of a magnitude large enough to suggest electoral realignment.

Note that these shifts are not temporary but persist from quarter to quarter. Yet they have nothing like the permanence envisioned in realignment notions. The partisan balance is not nearly so stable as *The American Voter* or critical realignment theory would lead us to expect of this "normal" postwar period. Instead, macropartisanship appears to be a midrange phenomenon, one that appears and disappears in a time frame of a year or two rather than a month or two or, alternatively, a decade or two. The movements within this stable alignment period appear substantial, both in magnitude and duration.

Is Macropartisan Movement Systematic and Does It Matter?

Before pursuing macropartisanship in earnest, we must first be sure that the movement we observe is more than the inevitable random fluctuations from sampling error. In principle, this question can be answered by a simple application of sampling theory to estimate the reliability of our aggregate measure. All we need is the average number of cases for our quarterly readings.

The average *N* is unavailable for the full set of Gallup surveys, because the provided number of cases in many instances is weighted by multiple counting of certain cases to achieve a representative sample. A conservative estimate, however, is a typical *N* of at least fifteen hundred partisans per quarterly reading. Given this approximate *N*, the average error variance is only about 1.67 percentage points. The observed variance for the quarterly series is 25.44. Dividing the former by the latter and subtracting from one yields an approximate reliability estimate of .93 for the time series.[3] Thus, the observable trends of the party identification time series cannot be accounted for by sampling or measurement error, a statistical conclusion that matches visual evidence.

What the series seems to indicate is that we have a phenomenon—multiquarter, multiyear systematic movements of partisanship—for which there is no obvious explanation. We have not tried but failed to account for it; instead it has gone pretty much unnoticed (but see Maggiotto and Mishler 1987). It is by no means a small matter. Given the often overwhelming causal power of micro party identification, knowledge of the secular movement of this macro series could give us purchase on all sorts of electoral phenomena.

The Electoral Importance of Macropartisanship

Party identification is a variable little in need of defense. At the individual level its explanatory power is thoroughly tested. But we might ask whether macropartisanship matters. That question, too, will provoke little skepticism. But it might be argued that the aggregated series contains no meaningful variation—that its movements are statistical flukes without consequences for the supposed stable patterns of U.S. party politics.

For an illustration that macropartisanship matters, we regress House of Representatives election outcomes (in Democratic seats won) on third quarter macropartisanship (expressed as percentage Democratic of two party identifiers). National House elections, both relatively stable and relatively partisan, should respond to underlying movements in partisanship. And they do. The regression shows that a one-point shift in partisanship yields a three-seat gain in House elections ($R^2 = .38$). Alternatively we can focus on votes instead of seats, where each one-point gain in partisanship is worth .31% of the national House vote ($R^2 = .23$).[4] Movements in macropartisanship do matter.

Party Identification Dynamics: Micro Level

We focus here on the partisan movement of the electorate rather than the more typical focus on the changing partisanship of individual citizens. To ask why the individual citizen sometimes changes identifications is a worthy question, the subject of a vast literature. But it is not our question. We wish to know about electorates, about net change.

To ask about net movements in partisanship is to ask only part of the micro behavior question, for we do not presume that the individual changes that produce net movements of one or two percentage points from one month to the next are more than a fraction of all individual partisan changes. But for the larger story of politics—the interaction of citizens and governing apparatus—they are the meaningful part. For that percentage or two has consequences; it builds or undermines electoral coalitions and it alters election outcomes.

Of course, the relationship between micro-level party identifications and our aggregated measure of macropartisanship can be a matter for interesting speculation. The systemic movements of macropartisanship do not by any means require that the citizens who comprise the electorate behave uniformly, that an increase, say, in proportion Democratic implies that each citizen individually becomes more likely to answer *Democrat* to the party identification query; for systematic macro patterns easily emerge from situations where only a relative handful behave systematically. It is the familiar story of aggregation gain. Where most are either fixed or changing in a noisy randomlike fashion and a few are systematic, the signal is wholly the behavior of that few.

One implication of this point is that our findings will neither support nor undermine particular models of individual partisanship. For those models are couched in the language of modal patterns and typical behaviors. Thus, an *American Voter* sort of model that posits partisanship prior to political evaluation could, for example, be fundamentally accurate with but a handful of exceptions. And yet that handful is enough to produce the macro behavior that we shall model.

Consider economic evaluations. A question often raised in the context of economics and politics is whether the average citizen can ever be adequately equipped with either the information or analytic tools necessary for economic evaluation. We don't know. Nor need we. For if we posited a hypothetical world in which, say, the daily subscribers to the *Wall Street Journal* alone made political judgments driven in part by economic performance, we would expect to see systematic movement in the aggregate.[5] It matters that *some* be capable of economic evaluation, not all.

This is little different from the original Downs (1957) formulation of the voter calculus. Our departure from Downs is agnosticism about whether rational and informed citizen behavior is typical or exceptional. Taking account of aggregation gain vitiates much of the three-decade conflict over citizen capabilities for rational action. The survey research tradition might be quite on the mark in asserting that average citizens do not so behave. But if only some do, the systemic consequences follow. "Thus it is quite possible," Converse similarly concludes, "to have a highly rational system performance on the backs of voters most of whom are remarkably ill-informed much of the time" (1986, 17).

Party Identification Dynamics: Macro Level

We now consider the causes of macrolevel movements in partisanship. The most obvious source for theoretical guidance is Fiorina's (1981) theory of cumulative updating. According to Fiorina's model, citizens use partisan orientation as a shorthand device for making sense of the political world. Citizens continually evaluate their political environments and adjust their views of the political parties accordingly. They alter their own partisan attachments as their comparative judgments of the parties' merits change over time. (More formally, this can be understood as a Bayesian updating model; see Calvert and MacKuen 1985).

The electorate's collective judgments about various aspects of the incumbent party's performance thus become the leading candidates to explain shifts in macropartisanship. When the incumbent administration fares well, its party should attract supporters. When the administration encounters disaster, it should lose its numbers. Our historical data allow us to test this proposition, as they provide periods of palpable good and bad times for both Democrats and Republicans. Gallup's measure of presidential approval is one obvious and important indicator of the incumbent party's perceived performance. Economic performance could matter too. But which economic indicator should we use?

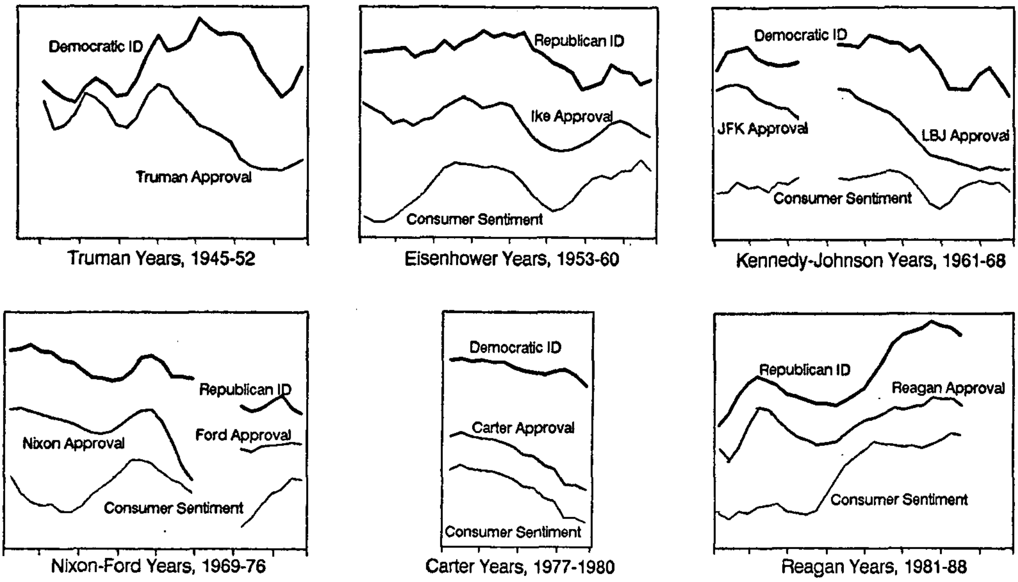
For a clean measure of citizen economic evaluations, we use the well-known (Michigan) composite Index of Consumer Sentiment (ICS). This index is available on a quarterly basis from 1953 onward. It taps perceptions of how well things have gone, are going, and (most important) are likely to go. It is a summary of the state of confidence citizens express in the economy. It is a short step to postulate a likely relationship between confidence in the economy and confidence in the economic managers—the president and his party. We presume—and have elsewhere (MacKuen, Erikson, and Stimson 1988) demonstrated—the index to be intermediate between objective economic indicators and political response. It taps the state of the economy as perceived by those same citizens from whom political response is expected. Clearly, it is a direct measure, purged of the usual slippage between what indicators show and what citizens feel. ICS is a composite of five separate items tapping retrospective and prospective evaluations of both the respondents' personal economic situation and the national economy.

We posit that macropartisanship responds to presidential approval and economic perceptions as registered by the Index of Consumer Sentiment. As numerous studies show (Hibbs 1982a, Kenski 1977, Kernell 1978, MacKuen 1983, Monroe 1981, Ostrom and Simon 1985, and others in support; Norpoth and Yantek 1983 in dissent), economic sentiment exerts its own direct effect on presidential approval. Thus a major portion of the effect of economic perceptions on macropartisanship may be indirect—with economic perceptions affecting presidential approval, which in turn affects partisanship.

The Correspondence of Consumer Sentiment, Presidential Approval, and Macropartisanship

We begin our analysis with a visual "test" of the responsiveness of macropartisanship to presidential approval and consumer sentiment. In Figure 2, we track partisanship (as support for the president's party), presidential approval,

Figure 2. Macropartisanship, Presidential Approval, and Consumer Sentiment: Truman to Reagan



Truman Years, 1945-52

Eisenhower Years, 1953-60

Kennedy-Johnson Years, 1961-68

Nixon-Ford Years, 1969-76

Carter Years, 1977-1980

Reagan Years, 1981-88

and ICS for 1946–86 (ICS from only 1953 on). Each is presented on a separate metric. In order that the eye not be distracted by random movement, this data display has been smoothed by taking a simple three-quarter moving average (the average of the preceding, current, and following quarter) for each time point.[6]

The data bear close inspection. It is clear that both approval and partisanship move in step with economic perceptions. Both rise with perceived prosperity and fall with perceived depression. The translation is sometimes loose. Not every twist in the economic series is mimicked in the partisanship series. Nor is it clear that the turning points in each series coincide exactly. As economists are apt to moan, it looks like the lags are long and variable.

Yet we should not lose sight of the overall pattern. At the level of, say, yearly movements, the consumer sentiment series appears to translate directly into both approval and partisanship. While the precision of a mathematical representation has yet to be demonstrated, the plausibility of modeling partisanship as a function of economic well-being is apparent. The relationship is evident to the naked eye. And even the loose translation is partly reassuring evidence. The notable mismatch of economic perception and political response, for example, occurs during the Johnson administration, where generally strong economic performance could not hold up political support in the face of foreign war and domestic turmoil, a pattern none will find surprising.

The pictures show that macropartisanship responds to historical forces. We need to know that the apparent relationships are more than optical illusions and that they reflect a plausible causal ordering. The first matter is measuring the extent to which macropartisanship coincides systematically with our measures of envi-

Table 1. Exogeneity Tests

	Independent Variable		
Dependent Variable	Partisanship[a]	Presidential Approval	Consumer Sentiment
Partisanship	—	.28 (.00)	.25 (.00)
Presidential approval	.12[b] (.18)	—	.27 (.00)
Consumer sentiment	.08[b] (.34)	.06 (.51)	—

Note: Entries are multiple correlations (root of R^2) for a model fitting the "dependent" variable to a simple AR(1)—"first order" in Box-Jenkins (1976) terminology—transfer function of the "independent" variable. Contemporaneous observations are omitted so that only previous observations of the "independent" variable are used. All variables are prewhitened by an ARMA process. Numbers in parentheses represent the probability that the correlation could be ascribed to chance.

[a] Partisanship is the percentage Democratic of the two party identifiers. Each of the remaining variables has been recoded (multiplied by −1) for Republican administrations so that the sign works in the expected direction. All variables are scored as mean deviates.

[b] The substantive coefficients of this model are of the wrong sign (negative).

ronmental conditions. After all, many tendencies fall to careful statistical scrutiny.

Fortunately, we can apply a formal "exogeneity" test to see whether the temporal correlations shown in Figure 2 can be attributed to genuine causal connections. A Granger-Sims test[7] proceeds as follows: First, each of the three series is whitened. The problem is that each series (as is typical in this sort of work) reflects its own previous values. The dependence process, an autoregressive function, is often similar in form across different series. It is the case that independent series with similar autoregressive functions will be substantially but artificially correlated over time. The solution is to *whiten* each series at the outset; that is to say, each variable is modeled as a function of its own previous values in such a way that the resulting series has (virtually) no autocorrelation. Each manifestation therefore represents the "innovation" in the series at that time point and does not reflect continuations of previous inputs.

Second, each variable is modeled as a transfer function of the previous *prewhitened* values of the other variables. For example, *whitened* partisanship is modeled as a function of previous values of *whitened* approval in order to see if the "innovations" in approval may be said to have caused subsequent innovations in partisanship. Post hoc propter hoc is no assurance of causality and its absence no sure disconfirmation, but our argument about meaningful relationships is substantially strengthened when such an exogeneity test is passed.

The results of the exogeneity tests are shown in Table 1. Here all possible causal connections are estimated, though of course some are theoretically implausible. Entries in the table are the correlations between the estimated model and the observed series. Equally important are the significance tests in parentheses. (Note that the table is asymmetric: temporal ordering makes a difference.)

Three connections show statistical strength. Consumer sentiment "causes" both approval and macropartisanship. Approval then goes on further, and independently, to affect macropartisanship. Our statistical apparatus also allows us to test the reverse causal flows. Happily, the evidence sustains none of the contrary linkages. Macropartisanship does not cause approval and neither macropartisanship nor approval shapes economic perceptions.[8]

Again, this more skeptical scrutiny sustains our understanding that macropartisanship varies in an interesting and theoretically meaningful way. This type of Granger-Sims exogeneity test can be overly tough (it is given to false negatives), so that our discovering substantial causal connections among the prewhitened variables is strong statistical evidence. The fact that it makes common sense is all the more appealing.

A Causal Model

All this suggests that we may be able to account for changes in macropartisanship with a substantively interesting empirical model. We should like to offer a plausible and interesting example of how this might be accomplished.

The preceding analyses were essentially bivariate in character. Yet it is clear that our variables are highly collinear. Thus, if we seek unbiased estimates of our empirical relationships, proper model specification is a matter of the first order.

With our major theoretical specification complete, we add to our quiver a couple of additional series. First, it is clear from models of presidential approval that specific political *events*, such as the Hungarian revolt and Suez crisis of late 1956, the Cuban Missile Crisis in 1962, and so on,[9] register in the public's psyche. What is not clear is whether they affect partisanship as well. One might guess that the connection would be less direct, but that is an empirical matter.

In addition, we include a set of *administration*-specific dummy variables to capture the public's long-term reaction to each particular presidency. This scheme has proven itself valuable in modeling presidential approval. Much previous work has suggested that this medium-term movement may be attributed to a dissolving coalition of minorities (Mueller 1970, 1973) or to a comparison with the failures of previous regimes (Hibbs 1982a, 1982b; Keech 1982). The importance for macropartisanship remains to be seen. For each administration we add a separate constant term and a template that begins with a score of one at the president's (initial) inauguration and then declines exponentially throughout his tenure in office. In order to avoid overfitting these specific data, the speed of the decline is here specified a priori from previous work (MacKuen 1983) better suited for this specific purpose, thus leaving only the magnitudes for estimation.

These two sorts of variables, the events and administration dummies, do not represent substantive theory (their specification is essentially ad hoc) but instead serve to avoid underspecification. This turns out to be important because our ability to get crisp estimates for the substantive variates depends on our not asking those variables to account for variances more directly attributable to the event and administration variables. Further, while not a priori measures of observable conditions, these variables are specified in a systematic fashion. To the extent that macropartisanship may be successfully modeled as a function of their manifestation, as well as that of the approval and consumer sentiment variables, our case is strengthened.

Putting all these pieces together requires two steps. First, we model presidential approval as a function of consumer sentiment, historical events, and administration dummies. Second, we model macropartisanship as a function of the *political* part of approval, as well as consumer sentiment, the historical events, and administration dummies. These successive stages are indicated by the model's recursive form. The results for the first part, presented in Table 2, show the sort of pattern obtained in much previous work. Importantly, we get a very crisp estimate of the effect of economic judgments on approval. Both the immediate impact (what would be an unstandardized regression coefficient in a contemporaneous analysis) and the dynamic coefficient are estimated with good precision. (The overall fit, an R-squared of .94 and a standard error of the estimate of 2.68, indicates that we have specified the model with some completeness). Getting good estimates here is important for the second step.

Approval is clearly a function of economic evaluations. We observe a direct translation of a shift of one point in consumer sentiment into a shift of .32 points in presidential approval. Thus, we need to eliminate the economic portion of approval to get clean estimates of the distinct effects of economics and of presidential approval. (In this sort of dynamic work collinearity makes simultaneous estimates pretty dicey). Here we generate a political approval series that is purged of the effects of consumer sentiment, but that includes all other variance components.[10] Thus, we may contrast the impacts of (1) economic conditions (measured by consumer sentiment) and (2) politics of the dramatic sort (measured by political approval with the economic component extracted).

Joining the components produces the estimates shown in Table 3.[11] First note the fit. A substantive model allows us to model 84% of the variance in partisanship over time.[12] This, of course, beyond what one might do by chance alone. Notwithstanding the usual provisos about overinterpreting goodness of fit, here there is an important message about our variable of interest: it must move quite systematically in order to be explainable by any model. Any hypothesis that the movement in partisanship over time is essentially random can no longer be sustained.[13]

But the fit is much better than that. Here the standard error of the estimate (1.83) is pretty close to a minimal sort of sampling error that one might expect from these data. Thus, another way of looking at the results is to guess that about five parts of six in macropartisanship's variance are substantively interesting.

We wish to do more than reject the straw man of randomness. These data suggest that a very large portion of the movement in macropartisanship is of substantive interest. The nature of the empirical estimates for consumer sentiment and for presidential approval encourage further understandings. The numbers are fairly large, are estimated with some precision, and are robust against alternative specifications (not shown).

Our dynamic specification requires interpretation to consider both how much influence each of the two variates has on partisan shifts and how that influence is felt over time—how quick the onset, how long-lasting the effect. The immediate impacts for both consumer sentiment (.10) and for approval (.22) are substantial. Roughly speaking, for every 10 people who move one unit on ICS (for example, from neutral to positive on all items), 1 of them changes parties in the next quarter. For every 10 who switch to or from approval of the president, 2 change parties.[14]

This immediate influence is easy to understand and appreciate. The long-range impact is more difficult to see. We need to turn to dynamics. The exponential declines—in this case estimated, not prespecified—for the impacts of consumer sentiment and approval allow for direct interpretation (see MacKuen 1981, chap. 2). Each represents a continuous process in which its initial impact dissipates or reequilibrates over time. The dynamics for each process may be characterized by a

Table 2. Presidential Approval (1953–87) As a Function of Consumer Sentiment, Events, and Administration Dummies

Variable[a]	Immediate Impact (ω_0)[b]	Dynamic Parameter (δ_1)[c]
Consumer sentiment	.32	.61
	(.04)	(.05)
Historical events		
Watergate	12.94	.28
Vietnam troops	-1.87	—
Iran crisis	15.07	.64
Reagan assassination attempt	-12.48	.77
Event series	5.10	—
Presidential administration intercepts and dynamic parameters		
Eisenhower	-.30	—
	-28.60	.85[d]
Kennedy	-1.38	—
	20.63	.85
Johnson	-12.86	—
	24.71	.85
Nixon	—	—
	-7.39	.85
Ford	10.96	—
	-22.98	.85
Carter	-19.13	—
	28.90	.85
Reagan	5.62	—
	-6.72	.85
Noise model		
Constant	.62	—
	(1.51)	
Disturbances	—	.33
		(.10)
Measures of fit		
R^2	.94	
Standard error of estimate	2.68	
N	140	

---

[a] Each of the variables has been recoded (multiplied by −1) for Republican administrations so that the sign works in the expected direction for the subsequent partisan analysis. All variables are scored as mean deviates.

[b] These are the scalar translations, equivalent to "regression" coefficients. For the critical variables the standard errors of the estimators are given in parentheses. Other coefficients are statistically discernible from zero.

[c] These parameters are the AR(1) transfer function parameters.

[d] This value and those below it are fixed a priori.

Table 3. Macropartisanship (1953-87) As a Function of Consumer Sentiment, Presidential Approval, Events, and Administration Dummies

Variable[a]	Immediate Impact (ω_0)[b]	Dynamic Parameter (δ_1)[c]	Time Constant (Quarters) (T_k)	Gain
Consumer sentiment	.10	.84	6.09	.59
	(.01)	(.02)		
Presidential approval (political)[d]	.22	.35	1.55	.34
	(.04)	(.09)		
Historical events				
Watergate	-5.69	—	—	—
Vietnam troops	.56	—	—	—
Event series	-1.38	—	—	—
Presidential administration intercepts and dynamic parameters				
Eisenhower	4.71[e]	—	—	—
	-5.11[f]	.85[g]	—	—
Carter	17.91	—	—	—
	-15.86	.85	—	—
Reagan	-5.26	—	—	—
	12.33	.85	—	—
Noise Model				
Constant	-2.78	—		
	(.46)			
Disturbances	—	-0.04		
		(.10)		
Measures of Fit				
R^2	.84			
Standard error of estimate	1.83			
N	140			

---

[a] Each of the variables has been recoded (multiplied by −1) for Republican administrations so that the sign works in the expected direction for the subsequent partisan analysis. All variables are scored as mean deviates.

[b] These are the scalar translations, equivalent to "regression" coefficients. For the critical variables the standard errors of the estimators are given in parentheses. Other coefficients are statistically discernible from zero.

[c] These parameters are the AR(1) transfer function parameters.

[d] Presidential political approval has the consistent component due to consumer sentiment's being removed before analysis. It represents the dramatic portion of presidential performance.

[e] Intercept.

[f] Decay parameter.

[g] This value and those below it are fixed a priori.

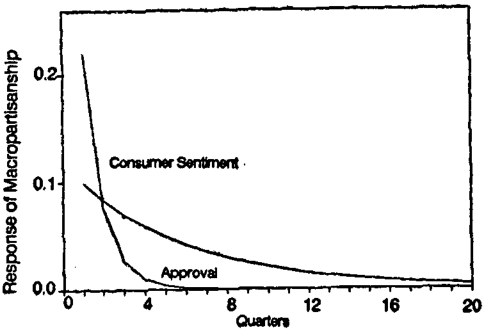
measure T_k, called a time constant or the mean lag, that fits the reequilibration speed for each variable to an empirical time scale. Formally, about 63.2% of the contemporary impact dissipates in the amount of time calibrated by one T_k. Thus, the estimated T_ks shown in Table 3 tell us how fast macropartisanship reacts to changes in approval and also how fast it reacts to changes in consumer sentiment. The time constant, $T_k = 1.0/(1.0 - \delta_{k1})$, is about six quarters (6.09) for consumer sentiment and a little more than a single quarter (1.55) for approval.

This means that current partisanship reflects (mostly) the impact of the current *quarter's* approval but the last *year-and-a-half's* economic conditions. Put another way, current economic conditions will continue to be felt for six quarters, but current approval will be mostly forgotten in six weeks. The difference in the persistence of each component's immediate impact is shown clearly in the upper half of Figure 3. This picture draws the response, over time, of a single-point, single-quarter shift in either consumer sentiment or approval. In this case the exogenous shift is followed directly by a return to the previous level—an impulse in exogenous change—and we see the reaction dynamics. Partisanship quickly "forgets" approval while it evinces a more elephantine memory for previous economic conditions.

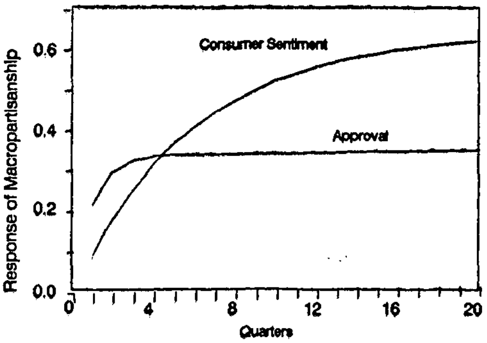
An equally useful way of seeing the same story lies in the equilibrium impact coefficient. This is the change in partisanship that would be produced if either approval or consumer sentiment changed by one point and then remained at that new level indefinitely. In this case, we have a "step function" as the exogenous change, and we calculate the response level reached in the long run.[15] This abstraction provides a more comprehensive view of empirical influence (*how much* taking into account *how fast*). Our estimates suggest that each one point change in approval ultimately results in about one-third of a one-point change in partisanship. In contrast, a point shift in consumer sentiment produces about .59 points response in partisanship. The equilibrium responses for the two variables are shown in the lower portion of Figure 3. Once we take into account response dynamics, it becomes apparent that economic influence is much greater.

In summary, the causal forces of politi-

Figure 3. Response of Macropartisanship to Changes in Presidential Approval and Consumer Sentiment

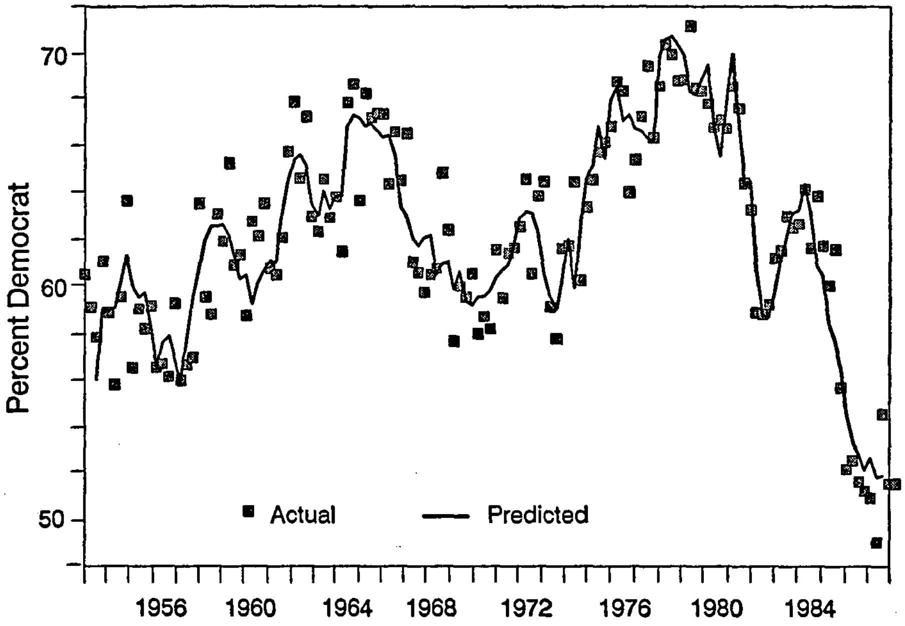


Response to Short-Term (for One Quarter) Unit Shift



Response to Permanent Unit Shift

Figure 4. Macropartisanship: Actual and Predicted



cal and economic evaluations have distinctive and substantial dynamic profiles. The impact of approval is sharp but transitory while that of economic evaluations is gradual and more enduring. Thus, assessing the relative contribution of politics and economics to macropartisanship is a matter of hare and tortoise. In the short run the impact of politics appears more important, but in the medium and long run the cumulative impact of previous economic perceptions becomes decisive. More significant for present purposes, the equilibrium impact of *each* of these causal variables is of appreciable weight. We have known for some years that approval and consumer sentiment rise and fall over time; it now becomes apparent that this movement translates into the dynamic of macropartisanship.

The main point of this exercise is to see that a substantive model can account for the evident fluctuations in partisanship. The fit statistics certainly confirm the message. But, of course, statistics can lie. Examine the graph in Figure 4. The series of solid black squares represents the actual readings on partisanship over time. The solid line tracks the prediction produced by our estimated model. It is clear that (1) the movement of macropartisanship is as complex as we would expect from the history of these four decades—this is no secular trend; (2) macropartisanship clearly incorporates factors that vary and vary irregularly. This is nothing like a realignment scenario. Gains and losses are "permanent" on a scale of months, not decades. Like economic cyclicality, there appears to be a regular back and forth dynamic to partisanship but with irregular amplitude and irregular duration.

On What We Know Now

We now know that partisanship moves and that the economy moves it. More precisely, we know that the aggregate division of partisanship has fluctuated over the past 40 years, that those fluctuations have been substantial, and that they have had political consequences. Finally, we now know that partisanship's twisting course has been shaped by the winds of political and economic fortune.

Knowing that the public's partisanship is subject to considerable variation forces us to reconsider the standard view of party systems and realignment theory. The dominant paradigm posits a stable self-maintaining party system that changes character only in sudden transfigurations. This theory is supported by the twin empirical regularities of stable partisanship in the individual's psyche and of stable partisanship in the aggregate distribution. While we do not question the centrality of partisanship within the individual's own political garden, we now perceive a very different place for partisanship on the collective political landscape.

More formally, the realignment view posits a punctuated equilibrium system: a system that yields a pattern of stable partisan conflict that only rarely—but dramatically—responds to changing historical circumstance. As normally understood, this system performance relies on the permanence of individual partisanship. A party system may withstand most political storms because individual citizens, who may be buffeted about momentarily, hold fast to their partisan ties. For the postwar United States the model predicts an essentially stable division in party loyalties, a stable division that is notably absent in our data. Instead, we discover that the partisan balance varied according to the political and economic performance of various governments.

The direction for further theoretical work is not obvious. As pre-Copernican astronomers preserved the Ptolemaic system, we may simply add medium-term partisan shifts to the longer-run cycles implicit in current party systems theory. The data mandate nothing more. By itself, this addition to party systems theory recasts our understanding about the flow of politics. The mid-range dynamics we highlight are of tangible importance. They yield partisan movements of realignment magnitude (though not realignment duration) that require neither miracles nor catastrophes but instead arise from the routine success and failure of ordinary politics. We argue for a quotidian, as well as a chiliastic, view of political change.

More speculatively, the dynamics of macropartisanship may indicate a deeper look at party systems theory. For some time now we have had considerable evidence that the ideological and social bases of the party division shift continually (e.g., Carmines and Stimson 1989; Petrocik 1981). We add our voice to those who argue that our theoretical challenge transcends that of cataloging electoral history into periods of realignment and periods of partisan stability. We must focus more clearly on the constancy of change. Rather than worry whether political changes are large enough to signal a realignment, we ought to wrestle with their cause and consequence.

Notes

This is a substantial revision of "Macro Party Identification: A Preliminary Analysis" presented at the 1988 Annual Meeting of the Midwest Political Science Association, Chicago. We thank Walter Mebane, Morris Fiorina, Warren Miller, Christopher Wlezien, and Richard Sobel for particularly valuable commentaries and Philip Converse for coming to our aid with data.

1. Sophisticated and forceful statements of the realignment literature can be found in Sundquist 1983 and Clubb, Flanagan, and Zingale 1980.

2. Allsop and Weisberg's (1988) demonstration that partisanship fluctuated meaningfully during the course of the 1984 election campaign is a notable exception.

3. This reliability estimate assumes a simple random sampling. Failure to approximate this assumption may cause the reliability estimate to err on the conservative side. As compensation, the assumption of an average *N* of 1,500 is probably overconserva-

tive. The quarterly Gallup readings are themselves aggregated from bimonthly readings. Measures of macropartisanship were obtained from the Roper Center as a systematic sampling of party identification from the first Gallup survey of every odd-numbered month. We reaggregated to quarters because key economic indicators are measured quarterly.

4. Predicting popular vote in contests for the presidency is yet another test, if a less desirable one (for want of cases). Here the translation is .55 points of the popular vote for a 1% change in macropartisanship with standard error .22 and $R^2 = .85$ for a model including also disposable income change and policy mood. In a bivariate (under) specification, the same coefficient (.56) is obtained, but with larger standard error (.44) and considerably lower explanatory power ($R^2 = .17$).

5. An alternative and wholly contrary scenario is that those minimally informed and minimally involved in political life learn the social consensus that one of the parties is doing a good or bad job with the economy and, in lieu of policy or ideological commitment, base their weakly determined partisanship on that knowledge.

6. Each of the three variables (Democratic macropartisanship, presidential approval, and consumer sentiment) is calibrated so that one unit represents the standard error of estimate from an equation predicting the variable from a series of administration dummies. Thus, each is measured as a deviation from the administration mean, with each having the same variance when summed across administrations. To avoid visual overlap of the three scales, 2 1/2 units are subtracted from approval and 5 units are subtracted from consumer sentiment. Please note that the smoothing and this rescaling are intended to make the timing and the dynamics clear to the eye. In the statistical analysis we employ the original series.

7. Exogeneity tests of this sort have received more attention in economics than in political science. See, for example, Granger 1969 and Sims 1977. For a good expository discussion in political science see Freeman 1983. Similar applications can be found in Norpoth and Yantek 1983 and Alt 1985. The models used here are simple linear filters: the persisting effects of the past are assumed to dissipate in an exponential fashion. Experience suggests that this simple model captures the main direct effects of most dynamic models.

8. This result is generated by a restricted form of exogeneity test in which the impacts of previous innovations are modeled to disappear in a smooth exponential fashion. We have examined these same propositions (among others) with a more powerful test, the unrestricted direct Granger test, and obtained substantively similar results (MacKuen, Erikson, and Stimson 1988).

9. Events are all coded in a single variable made up of unit impulses (that is to say, a set of zeros except at the designated time points where a score of one or minus one is substituted) and are thus treated equivalently. While this constrained estimation produces some inaccuracies, it avoids the trick of fitting dummies to error terms and calling it a model.

The pro-Republican events are Eisenhower's heart attack (3rd quarter 1955, 4th quarter 1955), Hungary-Suez (4th quarter 1956), Krushchev's visit to the United States (4th quarter 1959), the civil rights march on Washington (3rd quarter 1963), the Newark-Detroit riots (3rd quarter 1967), the Vietnam peace declaration (1st quarter 1973), the Mayaguez incident (2nd quarter 1975), the Achille Lauro terrorist capture (4th quarter 1985), and the Challenger explosion (1st quarter 1986). The pro-Democratic events are the army-McCarthy hearings (2nd quarter 1954), the KAL007 shoot-down (3rd quarter 1983), and the TWA hijacking in Beirut (2nd quarter 1985). Also added, separately, are the Iran Crisis (4th quarter 1979, 1st quarter 1980) and the Reagan assassination attempt (2nd quarter 1981).

10. This is simply approval minus that part of approval forecasted from the economic component alone, with other parts of the model zeroed out.

11. Note that the events and administration dummies are included for reasons of specification. The event series negative sign suggests that particular events are less compelling stimuli for partisan shift than for change in approval, as we might expect. The dummies for the Carter and Reagan administrations are not mere corrections for approval. They indicate some other phenomenon, perhaps a Watergate disillusionment of Republicans that was relieved with Reagan's assertive entrance stage Right. This component of the model, while systematic, is an explicit description of ignorance.

12. The R-squared is simply the squared correlation between the actual percentage Democratic and the predicted percentage Democratic, with the noise portion of the model zeroed out. Thus, the model is not self-correcting. Lagged values of the dependent variable do not appear on the right-hand side of the prediction equation.

13. These results are no artifact of our dynamic modeling. If we use ordinary least squares (OLS) and specify the same two models (for approval and macropartisanship), we produce a similar inference. The OLS equation for approval has, on the right-hand side, the same specification variables (the events and administration dummies) and also a single, lagged value for approval and for consumer sentiment. And a similar model for macropartisanship uses lagged political approval (actual approval minus .29 lagged consumer sentiment) and lagged macropartisanship (see Table N-1). These estimates are comparable to those in Table 2 and Table 3. These simple OLS models neither cope with autocorrelated disturbances nor allow different dynamics for approval and consumer sentiment. And their fit

appears inflated by including the measured lagged macropartisanship on the right-hand side. Nevertheless, their overall form shows that our substantive inferences are not mere technical wizardry.

Table N-1. OLS Models for Presidential Approval and Macropartisanship

Variable	b	SE (b)
Presidential approval[a]		
Presidential Approval (lagged)	.31	.05
Consumer sentiment (lagged)	.29	.04
Macropartisanship[b]		
Macropartisanship (lagged)	.26	.09
Political approval (lagged)	.13	.04
Consumer sentiment (lagged)	.10	.03

[a] R^2 = .94; Mean squared error = 3.14.
[b] R^2 = .81; Mean squared error = 2.17.

14. The immediate impact of economic evaluation may be larger than it appears here. Following convention, the consumer sentiment items are measured with a range of 2 points (0 negative, 1 neutral, 2 positive) while approval is scored as a dichotomy (0 or 1). One unit of ICS marks the movement from negative to neutral or from neutral to positive. One unit of approval reflects the complete change from a negative response to an approving one. Because these metrics are not the same, any claim about which variable shows a "bigger" impact on macropartisanship must be ambiguous. To make scores more nearly equivalent, the reader may want to double the coefficients associated with ICS.

15. Mathematically, for this simple linear filter, the equilibrium impact is simply $T_k + 1$ (the time constant [or mean lag] times the immediate impact). It appears that empirical estimates of the equilibrium impact are more robust than are the separate estimates of the immediate impact and how it gets distributed over time.

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PUBLIC GOODS, PRIVATE INTERESTS, AND REPRESENTATION

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We estimate a model of House members' roll call voting decisions embodying some hypotheses about representation, including estimates of the influence of district opinion on broad collective issues relative to personal economic interests, of the effect of electoral security on constituency responsiveness, and of the difference in constituency and party voting among Republicans and Democrats. This model is estimated with votes taken during deliberations on the 1978 Tax Reform Act, important because it was a significant change from the tax reforms passed in the late 1960s and 1970s, marked the first appearance of the Kemp-Roth proposed tax cut, and represented a concerted effort by Republicans to make tax policy a broad national issue. Findings indicate that constituent preferences for redistribution are important influences on representatives' decisions and that Republicans exhibited a greater degree of party voting than the Democrats while the Democrats better represented their constituent's preferences.

Nearing the end of the 34-hour marathon session that closed out the 95th Congress, the House let down its decorum. One young lawmaker donned comic glasses for a vote while just off the floor another played his saxophone. With the November elections less than three weeks away, representatives were eager to head home, but one prospective roll call stood in the way—a final vote on the Revenue Act of 1978. In a political environment where Proposition 13 was a recent and much ballyhooed revelation, this bill, granting substantial cuts in personal and capital gains taxes, was immensely popular. Debates were heated because the bill radically reversed two decades of changes making the federal income tax more progressive. Unusual as the setting may seem, 15 October 1978 was not atypical. Congress faces a befuddling array of expectations, conflicts, and constraints every day of every session.

Legislative scholars "explain Congress" with many heuristic schemes. We have learned about congressional campaigns (Fenno 1978; Jacobson 1983), norms and socialization (Asher 1973), committees (Fenno 1973; Hall 1987), roll call voting (Jackson 1974; Kingdon 1981), and constituency service (Cain, Ferejohn, and Fiorina 1987). The congressional subfield is eclectic, but two questions transcend specialties: Does Congress "represent" pursuant to collective interests or special interests? and Whose interests are they?" At a minimum, any assessment of representation must tackle the nexus between aggregate demands and the actions of "representatives," but it should also allow for the individualistic and stochastic world of comic glasses and saxophones.

We present evidence that votes of members of the U.S. House of Representatives reflect the mean preferences of their constituents, which in turn reflect the distribution of "public goods" preferences within the district. Further, the influence of constituency preferences is lower among representatives from more competitive and better-educated districts. We also find less response to personal and constituent considerations among representatives voting the "party line," who in this case were Republican.

Our analysis is not aimed solely at congressional scholars. A discussion about individual preferences for *public goods* distinguishes our use of this term from the sometimes ambiguous notions of the public interest or the common good. Further, we make several methodological advances. Whether a district elects a Republican or a Democrat reflects preferences both for a candidate *and* for certain policies. In other words, a lawmaker's party label and voting behavior are jointly determined outcomes in any representation model. Accordingly, our hypotheses are tested employing multivariate logistic equations. We also develop, using national survey data, a statistically consistent and more efficient method for measuring constituency preferences and for estimating representation than those used previously.

At its heart, though, we present a congressional voting model that lets us see how faithfully lawmakers represent constituent interests as the electoral competitiveness and the political sophistication of the district change. Past empirical examinations of representation take a different approach. A lawmaker's vote, we are told, is a weighted function of, for example, personal preferences of the representative + party affiliation + preferences of constituents. Unfortunately, many of the most intriguing theories of political philosophers cannot be tested within this additive structure. Is representation, for example, enhanced by having a politically sophisticated electorate? Such a question is properly modeled when the weight lawmakers give constituency preferences (e.g., how much influence constituents have over their lawmakers) varies with sophistication. In rough form, then, we hold that a lawmaker's vote is some function of personal preferences of the representative + demands of the party + (preferences of constituents × factors related to constituent influence). Factors related to constituent influence may include, among other things, the safeness of the seat, the electorate's sophistication, the homogeneity of the district, and the member's seniority.

We detail what is and is not implied by the concept of a "public good." We take issue with a well-known theme in the congressional literature—that lawmakers respond primarily to the narrowly defined and often selfish demands of their constituents. We detail why votes shaping the 1978 Revenue Reform Act are particularly timely and appropriate vehicles for testing theories of representation and develop a formal model of representation. We confront and resolve the methodological challenges brought on by estimating the public goods preferences among constituents in all 435 congressional districts. Finally, we report our empirical results and outline some of the implications of our findings.

Public Goods Preferences, Private Interests, and Redistribution

Our analysis of representation focuses on the concept of a public good and the distribution of people's preferences for public goods. The formal definition of a public good is a good that is available for, and consumed by, all individuals. Its consumption by any one person does not diminish its consumption by others. People cannot (easily) be excluded from sharing in its consumption and at times have no other choice (see Musgrave and Musgrave 1980, chap. 3).

Public goods preferences—such as what the income distribution ought to be, what cleaner air is worth, and how much national defense is desirable—are a vital part of people's political attitudes. (For evidence that individuals have measurable preferences for public goods, see Jackson 1983 on environmental quality and Hawthorne and Jackson 1987 on income redistribution.) Several tacts have been taken in measuring and talking about demands for public goods, and closely related but not identical notions include preferences for unitaristic policies (Wilson and Banfield 1964, 1971), symbolic politics (Sears, Hensler, and Speer 1979; Sears et al. 1980), and sociotropic attitudes (Meehl 1977; Kinder and Kiewiet 1981). Our concern is how much these preferences for collective outcomes, as distinct from private interests, are related to legislative decisions.

Public Goods and Private Interests

In the process of providing public goods, government programs also create private benefits and costs for specific individuals. Imagine workers for a major defense contractor. Weapons procurement contracts that provide the public good of national defense also affect their livelihood and are immensely important private concerns. Air quality emission regulations also produce private benefits and costs. As a consequence of cleaner air, some individual's property values will rise, firms who make emission controls will see increased profits, and frequent drivers and public utility consumers will have higher costs.[1] Or, with income redistribution, any change in the tax code lowers and raises taxes for selected individuals. Similar types of private interests are associated with all government actions to provide public goods.

An important issue in the public opinion and decision-making literature has revolved around a presumed conflict between private interests and the public interest or the public good. Often implicit in these discussions is the implication that collective behavior and decisions either promote the public interest or reflect the triumph of private interests. We agree that such tension exists, but for our purposes it is not the relevant distinction. We distinguish among three different terms and concentrate our analysis on two of these. The first is the concept of a public, or collective, good and the accompanying *distribution* of individual preferences about the proper amount of that good. Second is the normative discussion about the public interest or the public good, which usually debates public decisions according to some singularly defined objective and outcome, such as larger defense expenditures, cleaner air, or a more progressive income distribution. Lastly, there are the private, or particularistic, interests created by any governmental activity. Our concern is with the first and the third of these terms. Do congressional decisions, as reflected in the votes of individual representatives on specific roll calls, respond to and thereby reflect the distribution of preferences about public goods? Or do they primarily reflect the particularistic interests of constituents associated with each decision? Our normative argument is that the distribution of collective goods preferences should influence the behavior of political institutions, in this case, Congress. If we find evidence consistent with this argument, we leave it to others to judge whether that distribution and the congressional response lead to outcomes that agree with the reader's definition of the public good.

Preferences for Income Redistribution

Society's income distribution is a classic public good (see Thurow 1971). Redistribution preferences cannot be characterized simply as either altruism or self-interest but represent people's views of which changes in the income distribution are socially desirable. Individuals' redistribution preferences are related to their present and likely future income, as expected. However, there is far more to these preferences than just one's place in the income distribution. Other factors, such as age, race, region, and local economic characteristics enter into these preferences (see Hawthorne and Jackson 1987). People, even with the same income, disagree substantially on what changes are socially desirable. Support for redistribution is higher among blacks, residents in the East and Midwest, and people living in counties with high unemployment and high per capita income. Opposition to redistribution increases with age, education, and the proportion black within a person's county. The net result of these associations is considerable diversity of views as to how—or whether—the income distribution in our society ought to be changed, even among people with the same income.

Redistribution preferences play an important and central role in shaping individual positions on a range of specific tax policies, as should be expected of any public goods preference (see Hawthorne and Jackson 1987). Evaluations of capital gains exemptions, mortgage and gas tax deductions, and other similar proposals were strongly related to redistribution preferences as well as to whether the person was likely to benefit directly from the proposal. For example, homeowners who itemized deductions were more likely to favor the mortgage tax deduction than either nonhomeowners or homeowners who did not itemize deductions. However, among all three groups, support for keeping this deduction was strongly and negatively related to redistribution preferences. We interpret those results to mean that people evaluate tax reform with respect to its public goods (redistribution) implications as well as with respect to their own benefits. The question here is whether Congress does the same. If representatives promote only the private interests and neglect the public goods preferences of their constituents, congressional decisions will not fully represent the public's collective interests.

Representation in Congress

There exists a theoretical ideal point for governmental programs that reflects every individual's preferences for public and private goods. But actually defining society's collective preferences and assessing whether congressional decisions reflect these preferences is an impossible task. (See Chamberlin and Courant 1983 for an excellent theoretical analysis of this problem.) More often, congressional scholars examine dyadic-level relationships (between subpopulation constituencies and their single, elected representative) and ask whether constituency preferences are "represented" by lawmakers.

The lack of dyadic relationships does not preclude institutional representation. After all, conservatives in liberal districts may have their views advocated by conservatives from other districts (see Weissberg 1978 and note that we substitute *institutional representation* for his *collective representation*). Similarly, the presence of dyadic representation does not ensure institutional representation. Gerrymandered districts or representation of a biased set of preferences may preclude this result. However, the existence of dyadic representation increases the likelihood of institutional representation, which then depends upon the influence constituents have and the mix of preferences lawmakers reflect. For us, the critical question then becomes what public goods and private goods mix of preferences is reflected in Congress.

Qualitative studies of Congress offer some of the strongest evidence of constituency interests' influence on legislative decisions. Much of this literature emphasizes the importance of private rather than public interests. Mayhew (1974) argues that the electoral process induces lawmakers to legislate in issue areas that hand out "particularistic" benefits. Instead of reflecting national or collective goals, congressional outputs tend to mirror narrow, district-driven interests (on related issues, see Arnold 1979, Fiorina 1977, Lowi 1979, Shepsle and Weingast 1984). This district self-interest bias is enhanced to the extent that representatives respond to organized interest groups (Kingdon 1981, chap. 5) and to the extent that groups fail to mobilize around collective goals (Olson 1965). In short, Congress is often seen as much more likely to represent constituent preferences in self-interest areas (e.g., pork barrel) than collective goods issue areas (e.g., environmental protection, minority rights, etc.). Arthur Maass (1983) rejects this dominant caricature of Congress by detailing how legislative-executive interactions expand a program's "breadth of view"; but it is clear that the literature's image of Congress emphasizes the institution's adeptness at handing out private goods in order to increase the chance of reelection.

We contend that public goods representation is more likely than the dominant literature suggests. When evaluating various public actions, constituents consider both the likelihood of achieving some desired public objective and the action's impact on their personal situation, coming to a weighing of their utility for public goods and for private consumption. Lawmakers do not untangle their constituents' utility calculus, so representation of their voters' preferences produces attention to public goods. Only if there is some institutionally induced bias, as suggested by Mayhew, by Fiorina, and by Olson, will we see the pursuit of private interests and not find representation based on constituency preferences in the broader sense.

The emphasis on an institutionally induced bias for particularistic interests overlooks important evidence about how legislators relate to their constituents. When representatives talk about preferences in their districts, they often speak in general terms about broad issue areas. A district may be "hawkish," full of "tree huggers," "profarm," "anti-big business," and so on. Such generalities give lawmakers very strong cues about constituency preferences, which they may choose to follow when shaping legislation and voting in committees or on the floor. (Sears and his associates [Sears, Hensler, and Speer 1979; Sears et al. 1980] contend the public thinks about political issues in broad symbolic ways, which would lead to this process.) Furthermore, Fenno (1978) and Kingdon (1981, chap. 2) argue that representatives vote in ways they can "explain" in future campaigns. These explanations are likely to focus on the general topics that constitute constituents' frames of reference. The question then becomes whether these orientations evolve around public or private interests. If these general orientations center on broadly defined public goods, such as redistribution, environmental quality, or national defense, the incentives created by the explaining process will evoke attention to public goods concerns.

Revenue Act of 1978

What better way to observe the roles of collective and private interests than in efforts to change the tax code? The potential for self-interested behavior is evident, as individuals and groups fight for favorable rates and loopholes. Tax policy also taps into the distribution—and redistribution—of income within society. In 1978 over 10% of the GNP was devoted to income redistribution between private individuals. That is a monumental economic effort driven not merely by special interest demands but also by some agreement, and debate, about what the income distribution in society ought to look like. Tax policy also entails a choice among conflicting public objectives, which itself constitutes a collective decision.

The 1978 tax bill is a decade old and has been eclipsed by newer legislation, but our analysis of how pocketbook interests and redistribution preferences are represented is particularly timely. After a decade of big budget deficits, proposals to increase taxes are likely in the 1990s. What preferences will be reflected? Individuals' images of a socially desirable income distribution? Individuals' expectations of how tax policies can stimulate economic growth? Private concerns over after-tax income? Or some mixture of all three demands?

In 1978 lawmakers made several clear-cut choices in making the tax code less progressive. Republicans, as a party, made a conscious attempt to recast tax policy from a debate about income distribution to one over economic growth. Their proposals were aimed at stimulating new investment through changes in marginal rates and additional incentives. They wanted to reverse the increases in capital gains taxes passed in 1969 and 1976 and reduce marginal rates at the top end rather than at the low end, as had been the practice through the 1970s. The most radical proposal, sponsored by Representative Kemp (R-NY) and Senator Roth (R-DE), would have cut rates by 33% across the board. The efforts to defeat these Republican proposals offered legislators clear choices regarding both the national income distribution and the economic interests of their constituents. Finally, we examine the 1978 bill for a very practical reason: the necessary data to measure constituent preferences are available.

A reading of the *Congressional Record* leads us to expect that three of the House floor votes on 10 August 1978 shaping the 1978 tax bill may be interpreted as clearly "for" or "against" further redistribution of income or "for" economic growth:

1. *Vote on the Rule.* The motion was to order the previous question (ending debate) on the adoption of rule H Res 1306. The rule was adopted 284–130 with the Republicans voting as a nearly unanimous bloc, 136–8. While some Democrats were upset that the rule did not provide for a vote on a Social Security amendment, most debate surrounded the provision for a vote to recommit the bill with instructions to amend, as per Kemp-Roth, to cut individual taxes by one-third. As such, the vote on the rule became a first test of a representative's position on this radical tax plan, and a *nay* vote might be seen as proredistribution.
2. *Index Capital Gains.* The vote was on a committee amendment (sponsored by Texas Republican Bill Archer) to index capital gains taxes for inflation. The amendment was adopted 248–167 with the Republicans voting as a bloc, 142–1. Opposition was lead by Illinois Democrat Dan Rostenkowski, who argued that indexation would make the tax code more regressive because it amounted to a selective tax break for a wealthy minority. Archer described indexing as a progrowth hedge against inflation, and several supporters indicated that the code was already "too progressive."
3. *Corman Progressive.* The vote was on California Democrat Jim Corman's amendment to alter the planned tax cut to benefit taxpayers earning less than 15 thousand dollars. The tax cut would be paid for by taxpayers earning over 50 thousand dollars, thereby making the tax code more progressive. The amendment was rejected 193–225, and again, the Republicans voted as a bloc,

8–134. Debate quickly centered on just how progressive the tax code ought to be, and a *yea* vote might be interpreted as proredistribution.

Several other votes were taken, but, as debates recounted in the *Congressional Record* indicate, they were more concerned with procedural issues than income redistribution. First (362–49) and final (337–38) passage votes were bandwagon affairs because representatives did not want to be seen fighting (as contrasted with shaping) tax reform in the days of Proposition 13. However, *nay* votes here should reflect very strong opposition to the regressive plan contained in the 1978 act. The closest thing to a direct vote on Kemp-Roth was a proposal to recommit the bill to Ways and Means with instructions to add Kemp-Roth language. (We refer to this vote as "Kemp.") Rejection (177–240) reflected opposition by more than a few members who, as a matter of institutionalized discipline, would never vote to send a bill back to Ways and Means with instructions. Lastly, the House approved (268–135) a motion to instruct conferees to concur with a Senate amendment to reduce individual taxes by 5% annually in 1980–83 but only if federal outlays did not increase by more than 1% annually. This last condition made the vote largely symbolic, and some members balked at interfering with the conferees.

If we could (1) measure preferences for the public good of income redistribution within any congressional district, (2) develop a measure for districts' interest in the progrowth platform being advocated by the Republicans, and (3) find a proxy for the self-interest that a district may have for any particular tax code provision, we could learn something about the relative strengths of public goods and private interest pressures in forming this legislation.

A Formal Model of Representation

Representatives' voting decisions weigh institutional, personal, and constituent demands. Much of the voting decision literature assesses when, why, and to what extent one set of "pressures" predominates. For example, when are constituent interests (either public or private) weighed more heavily than congressional demands (such as party and committee obligations) or personal interests (such as those stemming from a member's seniority, ideology, or group identity)? Quantitative voting studies are designed to assess these weights, though the task has proved difficult. The voting decision is more complicated than a simple linear sum of relevant demands. Pressures interact, and our voting model must reflect verbal descriptions of these interactions.

Our formal model of the verbal hypotheses about legislators' decisions is nonlinear because some of the most interesting questions address the circumstances that strengthen the representation link between constituents and lawmakers. The model relates a representative's vote (V_i) to three components: institutional and personal factors ($X_{i,1}$), constituency preferences ($X_{i,2}$), and constituency influence ($\beta_{2,i}$). The *i* subscript in the last term indicates that we expect constituency influence to vary among legislators. In explicit form, this model is

$$V_i = X_{i,1}\beta_1 + X_{i,2}\beta_{2,i} + u_i.$$

In the next sections we describe the detailed specification.

Institutional and Personal Factors

Members' individual characteristics often guide their voting. These characteristics include both institutionally relevant factors, such as party affiliation and seniority, and proxies for personal preferences, such as race or age. The existence of party voting is well documented (see Collie 1983, 475–91 for a review of the literature). In this study, party is particularly important because the Republicans made tax policy and economic growth salient issues on which to campaign in future elections. The leadership worked hard, and with great success, to get members to vote as a bloc.

Seniority is a second institutional factor that may influence some voting decisions. More senior members may be unlikely to vote for proposals that violate House norms by, for instance, threatening committee autonomy and power. For example, on the initial vote on whether to adopt the rule, more senior members may vote affirmatively in support of the Rules Committee rather than because of their position on redistribution.

By all accounts, members' own policy preferences influence their voting decisions. How members think about society's income distribution will color their approach to issues that alter that distribution. Although we will not be able to measure a representative's preferences directly, we use race and age as proxies for these interests.[2] Hawthorne and Jackson (1987) show that these characteristics are highly correlated with redistribution preferences. Race may have an additional association with members' voting decisions because of the presence and importance of the congressional black caucus.

This individual part of the model is

$$X_{i,1}\beta_1 = B_0 + B_1\text{GOP}_i + B_2\text{terms}_i + B_3\text{nonwhite}_i + B_4\text{age}_i = B_0 + B_1G_i + B_2T_i + B_3N_i + B_4\text{ag}_i.$$

Constituency Preferences

Constituent positions are divided into three categories: their public goods preferences, their stake in the Republican's progrowth platform, and their narrow economic self-interest. Income redistribution is a classic public good and is central to any debate about tax policy. Redistribution preferences also capture the idea that constituents communicate general cues rather than specific demands to legislators. If representation is taking place, variations in redistribution preferences will be associated with differences in the probability of a representative voting for the Republican proposals.

The Republican strategy shifted the objectives of tax policy from income redistribution to promoting economic growth through individual investment incentives. This strategy should appeal most to people in growing areas. We proxy this appeal by the district's growth in personal income between 1969 and 1979, defined as the ratio of these variables.

Private interests are defined by how individual incomes will be altered by proposed tax changes. In this case, the Republican proposals to cut the taxes of higher-income people and to reintroduce the capital gains deduction provide clear indicators of self-interest. We represent these interests at the district level by percentage of the population earning over 25 thousand dollars. If self-interest is a strong factor in dyadic representation, the higher this proportion, the more likely representatives are to vote for the Republican proposals and against Corman. (The proportion with incomes below 5 thousand dollars is a second self-interest measure but is so highly correlated with the over-25-thousand-dollar variable that only one is necessary.) If this concept of self-interest relates to expected as well as current income, the income growth variable may be a second self-interest measure. These constituency preference hypotheses are shown symbolically as

$$X_{i,2} = C_1\%\text{inc} > 25_i + C_2(\text{inc }1979/\text{inc }1969)_i$$

$$+ C_3\text{redistribution}_i = C_1I_i + C_2\Delta I_i + C_3R_i. \quad (1)$$

Constituency Influence

The third part of the model represents the expectations about how constituency influence varies among representatives. The verbal descriptions relate these variables to the safeness of the member's seat, to whether the member is standing for reelection, and the sophistication of the electorate.

Fenno argues that large electoral margins leave representatives less constrained by constituent preferences, making it easier for them to evolve (or devolve) from a "constituent representative" into a "Washington representative" (1978, chap. 7). This statement implies that safeness is negatively associated with preference congruence. Kingdon, however, suggests that constituents reward responsive representatives with large reelection margins (Kingdon 1981, 67). He implies that safeness is positively associated with constituent representative preference congruence. These are not competing hypotheses. Kingdon is proposing the additional hypothesis that responsiveness, or congruence, may lead to the presence of a safe seat. These hypotheses need to be modeled by a separate equation explaining safeness.[3]

Safeness is measured by the member's plurality in the November 1978 election. Although this election follows the decision on the tax act, the roll calls all took place late enough for members to know whether there was a challenger and how strong the challenger would be. We could, alternatively, have used 1976 election returns, but we believe the 1978 numbers are a more realistic reflection of that fall's race. We expect that any simultaneity effects between votes on the tax act and the 1978 election results will be minor. For those with no challenger, the margin was set to 1.00. We also included a lame duck variable for members not running for reelection.

The level of political sophistication in a district may serve as a constraint on member actions and color how Washington behavior is "explained" at home (Fenno 1978, chap. 5; Kingdon 1981, chap. 2). The underlying proposition here is that constituent preferences (be they for public or private goods) are more faithfully represented in districts comprised of a politically sophisticated electorate. Political sophistication is measured by the mean years of schooling within the district. This choice can be justified on pragmatic and conceptual grounds. Pragmatically, years of schooling information is readily available for each congressional district. Conceptually, recent studies have shown that education has a strong association with other, more direct, measures of sophistication. (For an excellent discussion and evaluation of these measures, see Zaller 1986. He concludes that political information, or knowledge, is the best measure but that education performs well, is the second best measure, is highly correlated with information, and contains less measurement error.)

These hypotheses are combined into the following model of constituency influence:

$$\beta_{2,i} = 1 + A_1\text{safe}_i + A_2\text{lame duck}_i + A_3\text{education}_i = 1 + A_1S_i + A_2L_i + A_3E_i. \quad (2)$$

In the actual estimation of the model, we define safeness and sophistication relative to the average electoral margin (72%) and education level (12.2 years) among districts. We do this by subtracting these amounts from S and E in equation 2. This specification means that the coefficients denoted by C in equation 1 now indicate the constraint of constituency interests on a representative from this "average" district.

The Statistical Equation

These three separate parts are combined to form a single expression describing a member's roll call decisions,

$$V = B_0 + B_1G + B_2T + B_3N + B_4ag + (C_1I + C_2\Delta I + C_3R) \times (1 + A_1S + A_2L + A_3E). \quad (3)$$

In the first part of the model, each B indicates the expected change in support for an amendment or bill associated with a given characteristic. The coefficients denoted by C indicate the expected change in representatives' positions associated with a change in the relevant characteristics for districts with average safeness and sophistication. For example, C_3 indicates, for an average district, the association between redistribution demands and legislators' votes. Lastly, the coefficients denoted by A indicate how constituency influence varies with electoral margins and education levels. For example, we expect A_1 to be negative, indicating that constituency influence decreases with the safeness of the seat, and A_3 to be positive, reflecting that more sophisticated districts impose greater constraint on the actions of members. This last expression models constituency influence, necessitating the nonlinear, multiplicative form.

Methodology for Measuring Interests and Estimating Representation

Before discussing our results, it is critical to appreciate how difficult we as a discipline have found the problem of estimating constituent preferences, though not for lack of trying. Part of this difficulty has been conceptual, with researchers having different (and at times only implicit) definitions of constituent preferences or of what would constitute representation. However, a major hurdle has been methodological. Some significant efforts have been flawed so as to bias estimates, usually toward the inference of little representation of any sort. Others have relied on such tenuous proxies for constituency interests that conclusions about representation were highly speculative and unconvincing. This research improves on previous methodological efforts while simultaneously offering insights about the process of representation.

There are three methodological concerns in estimating equation 3. We will not dwell on these but summarize them and refer to various appendices and related papers. The first and most important concern is the measurement of constituency redistribution demands. Miller and Stokes (1963) estimated mean constituent opinion within congressional districts by sampling residents and averaging their responses to selected questions. This approach has a number of conceptual and methodological limitations that greatly restrict our ability to measure representation. (See Achen 1978 and Jackson 1989 for discussions of these limitations.) This approach is prohibitively expensive and will still only assess mean constituency opinion and representation with error.

Our procedure combines information from a survey assessing redistribution attitudes with district information to estimate mean constituency redistribution preferences. This method is described in detail and compared to other methods in Jackson 1989. The assessment of individual attitudes is done with data from a survey of attitudes towards tax policy conducted by the Roper organization in May 1978. This survey includes a specific question about using taxes to redistribute income.[4] (See Hawthorne and Jackson 1987 for an extended discussion of these attitudes and of the analysis of the Roper data.) The individual preference model estimated with the responses to the question in the Roper study is $\hat{R}_i = X_ib$. Based on this model of individual preferences, we estimate the mean district preference for district j by, $\bar{R}_j = \bar{X}_j b$, where $\bar{X}_j$ indicates the district means for the variables used in the model of individual preferences. The data on mean district characteristics, $\bar{X}_j$, were obtained from census files aggregating the data from the 1980 census into congressional districts for the districts in effect in 1978.

Identifying districts with the most and least favorable redistribution preferences provides some intuitive feel for this measure. The highest levels of support were among districts experiencing severe economic distress, such as black central city districts and areas with declining manufacturing industries. The least supportive districts were in Hawaii, Southern California, and the Rocky Mountain West. Almost as a rule, constituents in southern districts were less amenable to more income redistribution than their counterparts in the north. Nationally, the mean opinion in a district varied from about 44% to 78% in favor of income redistribution, and the average district mean opinion indicated about 57% approval.

This model-based method is preferred to the sampling-based method used by Miller and Stokes because it has smaller measurement errors, resembles the instrumental variables method for overcoming errors in explanatory variables, and permits the inclusion of all congressional districts in the representation model. Each of these advantages will provide less-biased and more-precise estimates of the influence of constituency redistribution preferences (see Jackson 1989 for further discussion).

The second methodological concern is that conventional logit or probit analysis will not capture the endogenous connection between representatives' party labels and their votes. It is well known that voters express policy preferences in the selection of a Republican or Democrat, meaning that a member's party is not an exogenous factor in any representation model. Still, despite Fiorina's (1975) gentle prodding, scholars have continued treating party as a given. We solve this problem by treating a district's party choice and a lawmaker's vote as part of a hierarchical two-equation system, with the implicit first equation predicting party affiliation. The choices of party (Republican or Democrat) and the lawmaker's vote (*yes* or *no*) are both binary, so either probit or logit methods are preferred to a linear model, such as two-stage least squares.

Our choice of estimation procedure for these two jointly determined dichotomies is based on the multivariate logistic model discussed by Nerlove and Press (1973) and Franklin and Jackson (1986). Denote the model of party affiliation as $P = Z\Gamma$ and the vote model given in equation 3, exclusive of party, as $V = X\beta$. (This general form includes, but ignores, the nonlinear aspects of the model.) We then denote by Q the joint interaction between party affiliation and vote decisions. Q measures how much more likely Republicans are to vote *for* regressive changes than against them and how much more likely Democrats are to vote *against* the changes, given identical personal, institutional, and constituency characteristics. The party and vote variables are combined to yield a nominal scale with four outcomes —Democrat and *no*, Republican and *no*, Democrat and *yes*, and Republican and *yes*. (All votes are polarized so that a *yes* vote favors the Republican plan and a *no* vote favors the proredistribution alternative.)

The statistical model is based upon estimating the probability a given representative falls into each category. The model's specification is such that the log of the ratio of these probabilities is linear in P, V, and Q. The log-odds model is specified to test the proposition that there is more party voting among Republicans, meaning they are less responsive than Democrats to the personal, institutional, and constituency factors in equation 3. This specification introduces a parameter, denoted as α, that measures the weight given to V by Republicans. With this specification, the log of the odds that a Democrat votes *yes* is $V - Q$, while the log of the odds of a Republican voting *yes* is $\alpha \times V + Q$.

The coefficients in equation 3 now assess how the log-odds measures change as personal, institutional, and constituency factors change. Since Q assesses the relationship between party affiliation and vote, if no association exists, controlling for V, Q will be zero. Positive values for Q indicate that even controlling for V, the log-odds of a Democrat voting *yes* are smaller than those of a similar Republican. Lastly, if α is one, variations in personal, institutional, and constituency factors, as expressed in V, have the same effect on the log-odds of a Republican voting *yes* as on a Democrat's. However, if α is much less than one, these variations have much less association with Republican votes. At the extreme, if α is zero, these associations are totally absent, and all Republican voting is simply party affiliation, as reflected in Q.

The third methodological consideration is the nonlinearities introduced in equation 3 by the hypotheses about variations in constituency influence. These specifications are incorporated into the statistical procedure so that we get direct estimates of all the coefficients denoted by A, B, and C in equation 3 as well as for Q and α in the log-odds model.[5] (Appendix A presents a more formal description of the model.)

Estimated Representation Model

Our results confirm that constituent demands—for public goods as well as for private goods—are important, though their influence varies far differently than was expected. The relationship between representatives' votes and constituent demands varied with the safeness and sophistication of the district on all votes but the Kemp recommittal motion.[6] But more often than not, less electoral safety and more political sophistication *decreased* the influence attributed to constituents. We will discuss these results in a moment. First, we want to explain how to read the tables, since our estimating procedures may be unfamiliar to some.

The entries in the tables reflect the expected change in the probability of voting for the Republican proposals associated with a change in each explanatory variable. The change in each explanatory variable corresponds to the range about the mean that includes one-quarter of the House members. For example, the mean proportion favoring using taxes to redistribute income is 56.7%, and one-quarter of the districts have proportions in the range 56.7% ± 2.4%. The table entries report the differences in the vote probabilities of a representative from a district where 59.1% of the residents favor redistribution with those of a representative from a district where 54.3% favor redistribution, everything else being equal.[7] The probabilities are calculated around the point $P = .5$ to maintain symmetry and comparability across votes. (The full statistical results are given in Appendix B.) Results are given first for the personal and institutional factors, then for the constituency variables and for the variations in their influence, and lastly for the partisan differences.

Personal and Institutional Influences

In a representative's voting calculus, the first set of influences described relate to personal preferences, associated with race and age, and to institutional pressures, defined here by senority. Table 1 shows the expected voting change associated with a change in each of these variables. Keep in mind that we have chosen as

Table 1. Relations with Personal, Constituency, and Party Variables

	Personal			Constituency			Party	
Vote	Term (2.4 terms)	Nonwhite	Age (6 years)	Income >$25,000 (1.6%)	Δ Income (13.4%)	Redistribution (4.8%)	GOP	GOP Factor[a]
Report	.09*	–.08	.07*	–.03	.08*	–.07*	.70*	.80
Capital gains	–.03	–.44*	.03	–.05	.05*	–.04*	.92*	.73
Corman	.01	–.30	.04	–.05	.12*	–.13*	.82*	.73
Kemp	–.06	–.23	.00	–.03	.04	–.09	.94*	.36*
Passage	.00	–.37*	.01	.05	.03	–.11	.86*	.39*
Conferees	–.12*	–.38*	–.02	–.09*	.04*	–.07*	.92*	.36*
Final vote	.01	–.32*	.01	.09	.08*	–.08*	.86*	.51*

Note: Entries are change in probability of voting for Republican tax proposals for given change in explanatory variable.

[a] Weight Republicans gave to personal and constituency variables, 1 = weight given by Democrats.

*Coefficient predicting this difference is more than twice its asymptotic standard error.

a representative range for these comparisons deviations about the sample means that include one-quarter of all members or districts. Seniority effects were only evident on the votes involving committee autonomy. A seniority increase of 2.4 terms is associated with an increase of .09 in the probability of voting for the report and decreases of .12 and .06 in the probability of instructing the conferees and of recommitting the bill, respectively. On all the other votes, which concerned only substantive matters, the expected probability change for this increase in seniority is .03 or less.[8]

Race was strongly related to voting. The expected difference in the votes of blacks and whites representing similar districts ranged from .08 for opposing accepting the report to .44 on indexing of capital gains, with most differences about .3. Nonwhite representatives always favored a more progressive redistribution of income than would be indicated by constituency preferences alone. A representative's age was related to voting, but not strongly so. Older members voted for regressive proposals more than younger members, as expected, except for instructing the conferees. However, these effects for a six-year age difference only exceeded .03 on two votes and .05 only for accepting the report. These results suggest that personal preferences related to race but not to age played a role in representative's voting.

Constituent Preferences

The proportion earning over 25 thousand dollars had virtually no association with representatives' votes on the tax reform act and was frequently negatively associated with votes. (Table 1 also gives the relations between the constituency variables and the probability of voting for the Republican measures.) Only on the two votes for passage, the most lopsided of all the votes, were the associations large and positive. On the vote on the Corman substitute, by far the most important of the votes, the association with voting *for* Corman was positive, though not significant. Thus, our most straightforward measure of self-interest was not systematically related to voting. Several other measures related to the income distribution were tried, such as percentage earning over 50 thousand dollars and over 15 thousand dollars, percentage earning less than 5 thousand dollars, and median income. All gave essentially the same results but with slightly poorer fits.

One explanation for the positive relationship between incomes over 25 thousand dollars and key votes might be that representatives from a few liberal, wealthy districts voted for redistribution. We made two attempts to ascertain if this was the case, both of which suggest not. The percentage with incomes over 25 thousand dollars was included in quadratic form to see if opposition to Corman increased over most of the range and then dropped in the upper levels. The result was just the opposite. Next, we reestimated the equation with the twenty wealthiest districts excluded. The estimated coefficient on percentage earning over 25 thousand dollars was virtually unchanged. The conclusion we reach is either that the income distribution variable is not a good measure of individual economic interests or that these interests are not particularly powerful explanations for representatives' decisions. We find the former conclusion surprising, given the nature of the issues being voted on, and the latter surprising too, given the prevalent views about legislative behavior.

The associations between income change and redistribution preferences and representatives' votes were consistently large and in the expected directions. The relation between income change and legislator's voting was particularly strong on the key votes on the report, capital gains indexing, and Corman and on the final vote. A 13.4% difference in income growth between 1969 and 1979 predicts a .12 higher probability of voting against Corman, a .08 higher probability of voting for the report, and a .05 higher probability of voting for capital gains indexing. We interpret these results as indicating both possible public and private interests. People with rising incomes may be less likely to favor income redistribution for self-interested reasons. At the same time, people in areas with increasing income may believe that stimulating economic growth is a more important public objective than income redistribution and accept the Republicans' argument that tax cuts are needed to stimulate that growth.

Increases in the proportion of the district that favors using taxes to redistribute income predict a significantly higher probability of that representative's voting against the Republican proposals. This relationship is largest on the Corman vote, where a shift of 4.8% favoring redistribution predicts a .13 higher probability of voting for Corman. The importance of redistribution preferences is indicated by the result that the differences in the probability of voting for redistribution measures associated with the 4.8% difference in public support were above .06 on all votes.

Variations in the Influence of Constituent Preferences

Safeness and sophistication were strongly related to how much district income change and redistribution demands constrained legislators' voting decisions but in precisely the opposite manner from what we hypothesized. Greater constituency education led to smaller associations between the constituency variables and legislator's voting behavior. On some of the important votes the differences were substantial. Larger electoral margins are associated with less constraint on all votes but the one for final passage. Table 2 shows the expected differences in the probability of a Democrat voting for the tax reforms for three different safeness and education levels for the same difference in redistribution preferences used in Table 1. An electoral margin of 72% and an education level of 12.2 years correspond to average safeness and

Table 2. Relations with Safeness and Sophistication

	Electoral Margin (% of Vote)			Education (Years of Schooling)		
Vote	.50	.72	.94	10.4	12.2	14.0
Report	-.04	-.07	-.10	-.13	-.07	-.01
Capital Gains	-.02	-.04	-.06	-.08	-.04	-.00
Corman	-.08	-.13	-.19	-.23	-.13	-.03
Kemp[a]	—	—	—	—	—	—
Passage	-.09	-.11	-.13	-.05	-.11	-.18
Conferees	-.04	-.07	-.11	-.09	-.07	-.05
Final Vote	-.10	-.08	-.06	-.16	-.08	.00

Note: Entries are change in probability of voting for Republican proposal for the same 4.8% change in redistribution preferences used in Table 1.

[a]Not applicable, as safeness and sophistication hypotheses are not significant.

sophistication levels and are the values used in constructing Table 1.

The Corman vote illustrates the interpretation of Table 2. With average electoral margins and education levels, a change of 4.8% in the proportion of the district that favored more income redistribution was associated with a .13 higher probability of voting for the Corman substitute (see Table 1). In competitive districts, where the electoral margin is only 50%, this expected probability difference is reduced to .08, while in districts with margins of 94% the expected difference is .19. In highly educated districts, with average schooling levels of 14 years, the expected difference is only .03 while in districts with an average 10.4 years of schooling, the expected difference grows to .23. (There are no entries for the vote on the Kemp recommittal motion, as these variations were statistically insignificant.) On all critical votes, higher electoral margins and lower education levels were associated with larger relationships between income change and redistribution preferences and the probability of voting for the Republican tax proposals. These results are statistically significant and contrary to expectations.

Some literature suggests that our result on safeness may not be so contrary. Powell (1982) also found a negative relation between safeness and representation. If more electorally competitive districts are more heterogeneous (defined as having a larger variance in redistribution preferences and a larger number of groups concerned about different issues), mean district opinion may appear less constraining if representatives follow nonmedian strategies in creating reelection constituencies from among various subgroups (Fiorina 1974). Another explanation for the contrary outcome of the safeness hypothesis is suggested in Kingdon's discussion that greater attention to constituency preferences may lead to a safer seat.[9] If this relationship is stronger than the rate at which representatives with safer seats deviate from constituency preferences, we are left with the observed positive association. (This is the classic simultaneous equation bias, only in this case it appears in models of the coefficients, which is far harder to rectify.) We have no obvious interpretation of the negative association between education and constituency constraint but do conclude there is no support for the sophistication hypothesis. We think safeness and sophistication are still very plausible hypotheses that warrant additional testing, and equation 3 offers the framework for such tests.

Partisan Differences

The differences in the voting patterns of Republicans and Democrats were larger than those associated with any other factor in the study. Republican and Democratic voting behavior can differ in two ways. One is the direct partisan effect, which predicts that a Republican's probability of voting for the tax reform proposals is greater than a Democrat's, everything else being equal. This partisan effect is represented by the extremely large interaction term in the model. The differences attributable to this interaction term are shown in the seventh column of Table 1, which show the differences in the probability that otherwise identical Republicans and Democrats would vote for each proposal (or against Corman). These differences were constructed so the effects of all other variables and constant terms sum to zero, leaving only the differences attributable to the partisan term. The magnitudes of these partisan differences is startling. The smallest partisan difference is .7, and all others are over .8! The clear implication here is that it would take considerable constituency influences or personal influences to induce a Republican to vote against the tax reforms or to get a Democrat to vote for them.

The second Republican-Democratic difference is our hypothesis that the efforts to create a cohesive Republican tax bill may have led to smaller associations between voting behavior and constituent and personal factors for Republicans than for Democrats. This hypothesis is assessed by a single coefficient reflecting the weight given to these factors. The coefficients shown in Table 1 under the GOP Factor column measure this aspect of party voting. In all cases, the relationship between constituency and personal factors and Republican votes was much smaller than the relationships for Democrats. On the first three key votes, the Republicans gave about three-quarters as much weight to these factors as Democrats, while on the other, more symbolic votes the differential dropped to just over one-third. These results indicate the Republican party leaders were very effective at building a cohesive party whose members gave less weight to constituency preferences and personal considerations and who voted as a bloc.

Why the Revenue Act Passed.

Our analysis has not answered the question, Why was there tax reform? There was little doubt after Proposition 13 and in a period of high inflation that a tax cut was in the offing. President Carter and congressional leaders of both parties proposed tax cuts. The critical question is, Why did *this particular* revenue act pass and depart so dramatically from previous reforms? The clues lie in the key votes that shaped the outcome of the reform, and they identify party and constituency as the perpetrators.

Republicans offered a well-defined tax plan to stimulate economic growth through cuts in the marginal tax rate and in capital gains taxes. Republicans voted as a near bloc to support this plan, both to get it on the floor and to kill more progressive Democratic alternatives. The Republicans' cohesive voting is clearly evident in the large GOP coefficients, indicating sizable partisan differences. However, this does not fully explain the outcome, as the Republicans were decidedly a minority party. There were 287 Democrats and 148 Republicans, so even with perfect cohesion the Republicans were still 70 votes short of a majority.

The Republicans needed Democratic votes to fashion a winning coalition. Republican strategists crafted a tax plan that won support from Democrats from districts with rising incomes and with constituents opposed to using taxes to redistribute income. The strong relationship between these two variables and lawmakers' positions on the key votes is evident in Table 1. Analysis of the votes on the critical Corman substitute give a clear picture of the importance of the constituency variables in explaining these defections. (On this vote a bare majority, 225–193, was against more redistribution. A switch of only 16 votes would have enabled Speaker O'Neill to cast the deciding vote, presumably in favor of Corman.) The equation estimated for the Corman vote predicts 148 Republican and 77 Democratic votes against Corman, enough to defeat his proposal. When we exclude the effects of income change and redistribution preferences from the predictions, there are only 8 predicted Democratic votes against Corman, far short of what the Republicans needed to pass their plan. This pattern was repeated on the report and capital gains votes, giving further indications of the important link between constituency interests and Democratic votes for the Republican proposals.

Republican cohesion broke only when party and constituent demands were forcefully at odds. Though only eight Republicans broke with their party on the Corman vote, all eight came from districts where the constituents strongly favored using taxes to redistribute income. On all the votes, though, there were many more Democratic than Republican defections; and the defections are strongly related to constituent demands. Without constituency interests and their relationship with members' votes, the Republicans would not have been able to build the winning coalitions needed to shape the tax act as they did.

Conclusions

Imagine 15 October 1978. Bleary-eyed lawmakers were nearing the end of a 34-hour session. Elections loomed large on the horizon. Representatives wanted to head home, but they stayed long enough to rewrite and redirect the U.S. Tax Code. Our study indicates that this redirection was greatly influenced by constituent public goods preferences, mobilized by the Republican party's electoral strategy.

This legislation was an important collective decision to make the tax code—and presumably the distribution of income—more regressive. Society's income distribution is pure public good, and constituent redistribution preferences were strongly associated with how representatives voted. Redistribution preferences represent people's perception of socially desirable changes in the income distribution. We also find, as expected, considerable disagreement about what changes are socially desirable. It is these disagreements over collective issues that we hoped to see, and saw, reflected in the decisions of our political institutions.

This legislation makes a second collective choice, beyond how to redistribute income. The Republicans framed the choice before Congress as one between two desired but competing public objectives—income redistribution and economic growth—not just as one between more or less income redistribution. To the extent that we cannot use tax policy to pursue both objectives simultaneously, we have a collective choice problem. People obviously disagree about the relative importance, as well as about the correct level, for each objective, so we must decide collectively how far to pursue each one. People more concerned about promoting economic growth will likely support the Republican proposals, while those who attach greater importance to a more progressive income distribution will side with the Democrat's alternatives. If the district income growth variable reflects, at least in part, the relative importance of economic growth to the people in each district, the large associations between votes and both of the public goods variables reflect congressional response to the distribution of preferences over this collective choice.

The notion of "representation within issue areas" is particularly interesting. Constituencies provide representatives with preference information about broad issue areas that the legislator can then translate into positions on specific proposals. For example, legislators need not know the precise percentage of potential voters supporting every suggested change in the U.S. Tax Code; they simply need to know how these voters think about income redistribution (or the importance of stimulating economic investment and growth). On a given proposal involving income redistribution, for example, representatives consider these general preferences, their importance, and how the change will affect individual incomes. They then weigh these considerations and vote accordingly. If elected officials must explain their votes on redistributive or growth issues in future elections, constituents' dispositions on these public objectives become integrated into how representatives approach tax issues. We suggest the same process will hold in other issue areas where constituents' preferences can be identified, for example, as proenvironment, probusiness, or antidefense spending.

Public goods considerations are central to constituent preferences and to legislators' voting decisions. This conclusion has substantial implications for questions about the character of representation. We cannot argue that House decisions in general (or on tax policy specifically) represent the collective will of United States citizens. We do contend that each member's votes strongly reflect mean district preferences for redistribution, preferences for the trade-off between the redistribution and growth objectives, and, to a lesser extent, preferences for personal economic interests. Our dyadic-level analysis has shown that at a minimum, House decisions do reflect the distribution of preferences on public goods existing within the country and are far more than an expression solely of "particularistic" interests. It is certainly conceivable that there is similar preference congruence on public goods issues at the institutional level.

We have not addressed the questions of representation off the House floor or by the Congress as a whole. How important are constituency preferences when lawmakers shape the legislative agenda, write bills, oversee the bureaucracy, or sit on committees? Are there biases in the processes of governance that make it more likely that certain types or sources of preferences will hold sway at different times? Are senators as sensitive to public goods interests as House members? Is tax policy somehow unique? Our finding that critical floor votes shaping tax policy reflect constituent public goods preferences makes research on these additional questions all the more intriguing. Our methodology for estimating constituency preferences and for modeling legislators' decisions provides a base for this research.

Appendix A. Formal Statistical Model

The statistical model has two nonstandard features—the inclusion of the representative's party affiliation as an endogenous variable and the nonlinear structure of the coefficients. The nonlinearities arise from the hypotheses about variations in constituency constraint and about Republican's response to personal and constituency interests.

Party is not exogenous in any representation model. It exists in a hierarchical system where constituency demands and characteristics affect the district representative's party, which in turn influences the votes of that member. This hierarchy is a two-equation system where the first equation models representatives' party affiliations and the second relates roll call votes to party, as shown in equation 3. This is the classical nonrecursive, structural equation model that makes it inappropriate to treat member's party as exogenous (Hanushek and Jackson, chap. 8). We model this system with a P (party) equation and a V (vote) equation, temporarily omitting party from the vote equation.

$$P = Z\Gamma$$

$$V = B_0 + B_2T + B_3N + B_4\text{ag} + (C_1I + C_2\Delta I + C_3R) \times (1 + A_1S + A_2L + A_3E)$$

A multivariate logistic model represents the relationship between the equations and the observed party affiliation and vote variables. This model accommodates both the dichotomous and endogenous nature of the two measures (Nerlove and Press 1973; Franklin and Jackson 1986). The two dichotomies constitute a single measure with four categories: Democrat and *no*, Republican and *no*, Democrat and *yes*, and Republican and *yes*. (All votes are polarized so that *yes* indicates a vote in favor of the Republican proposals, that is, a *nay* vote on Corman.) The interaction between party affiliation and vote is measured by a constant, Q, that increases the odds of being a Democrat and *no* or a Republican and *yes*. Lastly, our hypothesis that Republicans gave less weight to the personal and constituency factors in equation 3 is represented by the coefficient α that weights the whole expression for V for Republicans.

The logistic model then specifies the probability of being in each category as

Democrat and *no*	e^Q/D
Republican and *no*	e^P/D
Democrat and *yes*	e^V/D
Republican and *yes*	$e^{(P+\alpha V+Q)}/D$

where $D = e^Q + e^P + e^V + e^{(P+\alpha V+Q)}$.

This formulation makes party and vote endogenous with Q, the interaction between the two outcomes. Consider the predicted log of the odds of a legislator voting *yes*, given that she or he is a Democrat or a Republican. (In the logistic model these log-odds are linear, making exposition, but not interpretation, easier.) The log-odds of a Democrat voting are

$$\log\{Pr[\text{vote} = 1 \mid \text{Democrat}]/Pr[\text{Vote} = 0 \mid \text{Democrat}]\} = V - Q,$$

and the log-odds for a Republican are

$$\log\{Pr[\text{vote} = 1 \mid \text{Republican}]/Pr[\text{vote} = 0 \mid \text{Republican}]\} = \alpha V + Q.$$

The larger Q is, the less likely Democrats are, and the more likely Republicans are, to vote *yes*. The smaller α is, the smaller any association between personal and constituency factors and votes is, and the more likely a Republican is, simply to vote *yes*, for any positive value of Q.

Estimation of this model is based on the maximum likelihood function for the observed party and vote outcomes and selects the values of the parameters in the party and vote equations that maximize this function. In other words, we pick the estimates that are most statistically consistent with the observed data. The actual calculations were done with the program Statistical Software Tools (SST) developed by Dubin and Rivers (1987).

The variables used in the party equation, denoted by Z, are constant; South, East, and Midwest; education, log (population density), and Gerald Ford's 1976 vote percentage.

Appendix B. Full Statistical Results

Table B-1 presents the estimated coefficients and their asymptotic standard errors in the models estimating the prob-

Table B-1. Coefficients in Estimated Vote Models

Variable	Report	Capital Gains	Corman	Kemp	Passage	Confer	Final
Constant	-2.69	.75	-2.21	3.45	7.17	6.16	1.46
	(1.07)	(1.06)	(1.12)	(3.65)	(2.58)	(1.21)	(2.16)
Term	1.39	-.54	.18	-1.08	.08	-1.99	.11
	(.48)	(.48)	(.50)	(.64)	(.58)	(.47)	(.66)
Nonwhite	-.33	-2.67	-1.41	-1.01	-1.87	-1.95	-1.50
	(.53)	(1.15)	(1.29)	(1.07)	(.63)	(.80)	(.74)
Age	4.46	2.28	2.88	.26	.52	-1.23	1.74
	(1.70)	(1.72)	(2.05)	(2.43)	(2.07)	(1.76)	(2.29)
Income > 25	-.07	-.13	-.14	-.08	.11	-.22	.22
	(.07)	(.07)	(.09)	(.08)	(.08)	(.09)	(.11)
Δ Income	2.31	1.45	3.74	1.18	.79	1.24	2.41
	(.45)	(.39)	(.45)	(.69)	(.47)	(.33)	(.64)
Redistribution	-.06	-.03	-.11	-.07	-.10	-.06	-.07
	(.01)	(.01)	(.02)	(.04)	(.03)	(.02)	(.03)
Margin	2.07	2.42	1.91	—	.82	2.28	-1.37
	(.89)	(1.23)	(.63)		(.34)	(1.26)	(.89)
Lame duck	-.43	.99	-.35	—	-.30	-.74	.44
	(.34)	(.75)	(.26)		(.15)	(.49)	(.33)
Schooling	-.47	-.58	-.41	—	.31	-.19	-.57
	(.21)	(.35)	(.14)		(.12)	(.19)	(.23)
Party—Q	1.76	3.22	2.30	3.37	2.63	3.15	2.59
	(.24)	(.64)	(.25)	(.49)	(.70)	(.62)	(.95)
$\alpha \times$ GOP	.80	.73	.76	.36	.39	.36	.51
	(.12)	(.15)	(.14)	(.27)	(.20)	(.14)	(.20)

Note: Asymptotic standard errors are in parentheses.

ability of voting for the Republican tax proposed or against the Corman substitute.

Notes

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1. See Polinsky and Rubinfeld 1977, on the relationship between air quality and housing prices.

2. It is commonplace to use interest group ratings, such as those of the Americans for Democratic Action (ADA), to proxy member preferences. Unfortunately, adding such a vote score to our list of explanatory variables is, in effect, regressing votes on votes. We reject that maneuver outright. For a vote score to reflect a member's preferences faithfully, one must assume that every component vote accurately reveals only the member's *personal* preferences. If, on any component vote, members were responding to a mix of pressures, such as party, constituent, and institutional demands, at odds with their individual preferences, the vote score is an error-filled measure of personal preferences. Worse still, these errors are not white noise but contain systematic biases. Inclusion of a vote score variable with these systematic errors will bias coefficients estimating the independent effects of party and constituents, as well as of ideology. For these reasons, vote scores, including the ever popular ADA and Americans for Constitutional Action ratings, should be expressly avoided in analyses such as this. See Jackson and Kingdon 1989.

3. If both Fenno's and Kingdon's predictions are correct, as we suspect, we have a nonlinear simul-

taneous relationship between representation and safeness. Estimation of such a structure is beyond our scope here.

4. The precise question is, "Aside from raising money, the taxing system in our country has come to be used for a number of purposes—to redistribute income, . . . , etc. Here is a list of some different things. Would you tell me for each one whether you think it is something that should be done through raising or lowering taxes or done through other means or not done at all? . . . to reduce the amount of money those who are better off have, and increase the amount of money poor people have?"

5. The nonlinear specifications are incorporated in the maximum likelihood function for the multivariate log-odds model and in the first-order equations used to find the maximum of this function.

6. Statistical tests of the significance of different parts of the model were done comparing the values of the log-likelihood function for different specifications. -2 times these differences are distributed as a chi-squared distribution, with the number of degrees of freedom determined by the number of coefficients being deleted. The first test compared the model with safeness and sophistication, equation 3, to one without these terms. On all but the Kemp recommital vote the values of the chi-squared statistic far exceeded any conventional critical value, leading to rejection of the hypothesis that the relationship between roll call votes and the constituency variables did not vary with electoral margin and education. On the Kemp vote, the chi-squared value was 5.94 and the critical value for $\alpha = .10$ is 6.25. The second test compared the model with and without the constituency variables. The null hypothesis that the constituency variables had no relationship with votes was rejected in all instances.

7. This expositional format is the simplest way to summarize the statistical results because the coefficients in the logistic model are difficult to interpret directly. These distances are chosen arbitrarily and simply for illustrative purposes.

8. We also examined whether members of the Ways and Means Committee voted differently from other representatives and found that they did not.

9. See n. 3.

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LEADERSHIP EFFECTS IN PARLIAMENTARY ELECTIONS IN AUSTRALIA AND BRITAIN

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Political party leaders are an increasingly influential electoral force in contemporary liberal democracies. We test the hypothesis that their appeal is idiosyncratic, that is, that their electoral effect is a function of the leadership qualities voters perceive individual candidates as possessing. Thus, the less similar their personality profiles, the more the characteristics influencing the vote should differ from one leader to another. A comparison of Australia and Britain finds the opposite to be the case. Despite the divergent profiles of party leaders, the precise characteristics influencing the vote are remarkably similar in the two countries. This does not mean, however, that variation in the distribution of these characteristics is unimportant. It can affect the balance of the party vote and may even have been the difference between victory and defeat for the Australian Labor party in the closely fought 1987 election.

Political party leaders are increasingly the centerpiece of the party battle in contemporary liberal democracies. With television's ever more important role in the dissemination of political information and the structuring of political discussion, they are among the principal means by which political parties project themselves and shape their popular images. This is hardly a novel observation in a United States whose constitution militates against party government. It is a development of relatively recent vintage, however, in parliamentary democracies traditionally characterized by strong party government. The tendency to label governments by the name of their leader rather than of the party seems, for example, to have become especially pronounced in Britain since Margaret Thatcher became prime minister in 1979.

In the United States, this "leadership effect" has long been recognized as the strongest force for short-term change in the distribution of the presidential vote (Stokes 1966). Moreover, it has become stronger since the 1950s (A. Miller and W. Miller 1975, 422). But early voting studies in parliamentary democracies like Australia and Great Britain tended to downplay the independent electoral impact of party leaders, arguing that they were largely indistinguishable from more generalized party images (Milne and MacKenzie 1954, 1958; Rawson and Holtzinger 1958). More recently, though, this conclusion has been revised as party leaders have been shown to have a significant influence on how people vote (Bean and Kelley 1988; Butler and Stokes 1974; Graetz and McAllister 1987a, 1987b; Mughan 1978).

The question that logically suggests itself is, What personal qualities make political leaders an electoral asset for their party? Moreover, are these qualities

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unique in their electoral effect to particular individuals or can other party leaders expect to reap similar rewards by being perceived to possess them? By showing that presidential candidates are evaluated according to criteria that by and large do not vary from aspirant to aspirant and from election to election, research on the United States suggests an affirmative answer to this second question. Voters would seem to approach an election with a well-defined mental image, or schema, of what a president should be like; and they evaluate candidates against this image. The characteristics of candidates that affect how people vote thus tend to be stable (Kinder et al. 1980; Miller, Wattenberg, and Malanchuk 1986; Kessel 1988).

It is not certain, however, that this characterization of the nature of leadership appeal is generalizable beyond the U.S. presidential system. Previous studies of party leaders in parliamentary systems do not address this issue since they have detailed the content of their images without relating it to voting behavior (Aitkin 1982; Butler and Stokes 1974). But more to the point, prime-ministerial candidates remain more closely identified with party labels than presidential candidates and do not enjoy the same visibility and importance in the electoral process. Popular expectations of what they should be like can thus be expected to be less well developed, and the criteria on which the public judges them more idiosyncratic; that is, to the extent that party leaders are perceived to vary in the qualities they possess, those qualities having an electoral impact should differ from one leader to another. In testing this hypothesis, our basic strategy is to maximize the opportunity for idiosyncratic personality characteristics to come to the explanatory fore by comparing two countries, Australia and Great Britain, whose voters differ in their degree of partisan alignment and whose prime ministerial candidates contrast sharply in both their popularity with the electorate and their personality profiles.

Australia and Great Britain

Reflecting a common cultural heritage, the Australian and British party systems have developed along very similar lines and are perhaps best known for being structured by high levels of class voting relative to other Anglo-American democracies (Alford 1963; see also Kelley and McAllister 1985). In more recent years, however, the developmental trajectories of the two-party systems have diverged. In particular, the influence of social class has weakened in British electoral politics and among the consequences has been the emergence of an electorate more responsive to short-term forces and hence more volatile in its voting habits. The Australian party system, in contrast, has remained largely unchanged over this same period. Popular identification with the major antagonists in elections—the Australian Labor party (ALP) on the one hand and the conservative coalition comprising the Liberal and National parties on the other—has changed little over the last two decades or so, the electorate has become no more volatile in its voting habits, and the same three parties continue to dominate the federal vote (Aitkin 1985; Bean and Kelley 1988; Crewe 1985; Franklin 1985; McAllister and Ascui 1988; Mughan 1987).

A second pertinent difference between the two countries concerns the popularity of their major party leaders. The elections to be compared are the Australian federal election of July 1987 and the British general election of June 1983. Bob Hawke, the ALP leader, won national office for the first time in March 1983 and was reelected in December 1984. His British counterpart, the Conservative party's Margaret Thatcher, first became prime minister in May 1979. At the time of the relevant

Table 1. Mean Midterm Popularity Ratings of Party Leaders and Parties by Country

Leader or Party	Australia	Britain Total	Britain Pre-Falklands	Britain Post-Falklands
Hawke/Thatcher	53	38	31	46
Howard/Foot	33	21	23	18
Labor/Labour	44	33	35	30
Liberal/Conservative	34	36	30	42

Note: These percentage figures are calculated over the months during which Howard and Foot were the opposition party leaders in their respective countries.

Sources: The British figures are calculated from the Gallup Poll data in David and Gareth Butler, *British Political Facts, 1900–1985* (London: Macmillan, 1986), 263–64, and the Australian ones from various issues of the Morgan Gallup Report.

election, both prime ministers had thus been in the public eye as their country's leading political figure for a period of about four years. Both also entered the election facing a new opposition party leader. In 1987, Hawke faced John Howard, who had become the Liberal party leader in September 1985, whereas Michael Foot, having become Labour leader in November 1980, was Thatcher's 1983 opponent.

Table 1 summarizes the midterm popularity ratings of these four prime-ministerial candidates. While both incumbent prime ministers enjoyed much the same margin of advantage over their respective opposition party leader, Hawke was substantially more popular with Australians as a whole than was Thatcher with Britons. His superiority in this regard was particularly marked before the Falklands War did so much to boost Thatcher's personal standing in the polls (Norpoth 1987). Even this foreign policy triumph, however, was not enough to close the gap between the two prime ministers. The second, and more telling, measure of Hawke's exceptional popular appeal is the size of the lead that he enjoyed over his party. It averaged out at nine percentage points, while Thatcher's was negligible before the Falklands intervention and climbed to only four percentage points after it.

This analysis, then, compares the electoral effect of leadership characteristics in the substantially different contexts of Australia and Britain in the mid-1980s. The two party systems had been moving along divergent paths for over a decade and the incumbent prime ministers not only differed substantially in their level of personal popularity but also headed parties from opposite sides of the political spectrum. Furthermore, and as will become apparent presently, voters attributed very different personality profiles to them. Thus, if party leaders in parliamentary systems are indeed judged by personalized, idiosyncratic criteria, the leadership qualities having an electoral effect should be markedly different in the two countries. Before this hypothesis can be tested, however, various methodological issues need to be considered.

Data and Analytic Methods

The data used in this analysis come from the 1983 British Election Study and the 1987 Australian Election Study. These are nationwide, postelection surveys involving samples of 3,955 and 1,825 respondents respectively (see methodological appendix for further details). Largely because the British survey was conducted through face-to-face personal interviews and the Australian one was a self-completion mail survey, there are some minor differences in the wording of the questions used in this analysis. These differences prove unimportant for the most part and those meriting further comment are dealt with at appropriate points along the way.

The dependent variable is the vote at the relevant election (in the Australian case, the vote for the House of Representatives), scored 1 for the party in government (Labor in Australia and Conservative in Britain) and 0 for the party in opposition (Liberal-National in Australia and Labour in Britain). Minor parties are coded .5. When party identification enters the analysis as a control variable, it is scored in the same way. The main independent variables comprise a battery of personal characteristics, or qualities, that are commonly thought desirable in political leaders in both countries. In order of their presentation in both survey instruments, these are *caring, determined, shrewd, likable as a person, tough, listens to reason, decisive,* and *sticks to principles.* Respondents were asked which of these qualities, in their opinion, the party leaders possessed, it being emphasized that they should choose as many from the list as they thought appropriate for each leader. Their choices are operationalized as a series of dichotomies, scored 1 whenever the characteristic is mentioned and 0 otherwise. An additional characteristic, *effectiveness,* figures in both surveys as a separate question (see Appendix).

A question requiring careful consideration is, Are individual leadership characteristics better operationalized separately or as a combination of perceptions of the opposing leaders? The argument for their separate treatment might be thought strongest in the relatively consensual U.S. political culture; and, to be sure, many studies use separate measures of popular attitudes towards individual presidential candidates (Markus 1982; Stokes, Campbell, and Miller 1958). In other studies, by contrast, the Democratic and Republican candidates are treated as opposite sides of the same political coin and their electoral standing is expressed in terms of the net balance of opinion toward them either in the aggregate or decomposed into separate components (Kelley and Mirer 1974; Miller and Levitin 1976; Miller, Wattenberg, and Malanchuk 1986). In the more adversarial Australian and British political cultures, it might be thought that this second measurement strategy is more appropriate, since their party leaders are more likely to be seen in zero-sum terms. For a respondent to think of Hawke as caring, in other words, would preclude him from assigning the same quality to Howard.

But how party leaders are perceived is ultimately an empirical question, albeit with important substantive implications. Table 2 presents data that speak directly to this issue. Columns 1 and 4 display the product-moment correlations between prime minister and opposition leader on each leadership quality for Australia and Britain respectively. These correlations suggest that attitudes towards the party leaders are not zero-sum (also see Appendix). Far from being perfectly and negatively associated, the perceived qualities of competing leaders turn out hardly to be related at all in either country. Indeed, there are a few instances (three in Australia and one in Britain) where individual respondents show a slight tendency to attribute the same personal qualities to both major party leaders, making the intercorrelation of these items positive. Thus, each of the four party leaders can properly be treated as an electoral force in his or her own right.

There is one potential qualification to this conclusion and it is that leaders' profiles may be largely a function of respon-

Table 2. Correlations of Leadership Characteristics by Country

	Australia			Britain		
Leadership Qualities	Hawke with Howard	Hawke with Party Identification	Howard with Party Identification	Thatcher with Foot	Thatcher with Party Identification	Foot with Party Identification
Caring	-.03	.46	.29	-.07	.30	.25
Determined	.12	.18	.15	-.01	.18	.15
Shrewd	-.10	-.20	-.18	.02	.08	-.02
Likable as a person	-.12	.42	.27	-.08	.24	.22
Tough	.12	.12	.02	-.04	.04	.08
Listens to reason	-.14	.46	.35	-.09	.26	.31
Decisive	.09	.22	.16	-.01	.26	.15
Sticks to principles	-.08	.36	.29	-.07	.24	.19
Effective	-.20	.45	.48	-.16	.28	.39

Note: For each leader, party identification is scored 1 for the leader's party, 0 for the major opposition party and .5 for other or no party identification.

dents' party identification. There is a known tendency for identifiers to view their party's leader in a favorable light, and it is possible that we have only shown that negative partisanship does not lead them to view the opposition party leader in an equally unfavorable light. This being the case, people's identifying with a party could still structure their perception of leaders, making it misleading to treat these perceptions as independent electoral forces. This fear proves ungrounded, however. Columns 2, 3, 5, and 6 of Table 2 detail the simple correlations between partisanship and the personal characteristics of the party leaders separately. The various qualities can be seen to correlate with party identification to differing degrees for different leaders. Moreover, they average out in absolute terms at only .32 for Hawke, .24 for Howard, .21 for Thatcher, and .20 for Foot. Thus, while the interrelationship cannot be denied, it is nowhere near strong enough either overall or in individual cases to show perceptions of leaders' qualities to be artifacts of partisanship, or vice versa.

The personality profiles of prime minister and opposition leader in Australia and the same profiles in Britain, then, are substantially independent of each other and of those leaders' positions as party leaders. The questions now addressed concern first the substance, or content, of these profiles and second, their relationship to voting behavior.

Personality Profiles: Content and Electoral Effect

Table 3 details the distribution of leadership qualities for Hawke, Howard, Thatcher, and Foot separately. Its immediately striking feature is dissimilarity both within and between countries. Most obviously, there is no group of characteristics that can be called "winning" or "losing" characteristics. Insofar as the winners are credited with more leadership qualities on average in each country, they are distinctive only for having the better defined public profile. It is impossible with these data, though, to say whether this difference is a function of Hawke and Thatcher as individuals or as incumbent prime ministers. The fuller profile of the victorious leaders, in other words, cannot

Table 3. The Distribution of Leadership Qualities by Country

Leadership Qualities	Australia		Britain	
	Hawke	Howard	Thatcher	Foot
Caring	42	35	14	46
Determined	68	55	84	20
Shrewd	66	31	52	19
Likable as a person	51	30	13	35
Tough	48	22	79	8
Listens to reason	36	30	15	40
Decisive	36	20	60	8
Sticks to principles	25	31	72	25
Effective	80	52	91	30

Note: Each cell entry is the percentage of respondents attributing the particular quality to the leader in question.

be taken to suggest a systematic difference between winners and losers; it may represent instead an incumbency effect.

A noteworthy dimension on which Hawke and Thatcher in particular differ from each other is the polarization of their profiles. The British prime minister is seen as having certain qualities by more than three-quarters of voters (effectiveness, determination, and toughness) and as having other qualities (listens to reason, caring, and likable as a person) by fewer than one in six of them. Hawke's image, by contrast, is less sharply drawn. He is credited with only one quality (effectiveness) by more than 75% of voters; but there is also only one (sticking to principles) that fewer than one in three see in him. Thatcher might, therefore, reasonably be expected to be the more powerful electoral stimulus.

Overall, the differences in the leadership profiles of the four party leaders clearly outweigh the similarities, and this is especially true for the two prime ministers. By any definition, Hawke and Thatcher are exceptionally successful political leaders; and to the extent that their personal qualities influence voting behavior, Table 3 would suggest that they have been deemed successful for different reasons. At first glance, this observation supports the hypothesis that leadership appeal in parliamentary systems is unsystematic in the sense of being founded on qualities peculiar to individuals. Yet as compelling as this conclusion may be on the basis of the evidence so far, bear in mind that the distribution of personal qualities and their effect on the vote are two quite different things. It matters little, for example, that Thatcher is widely perceived to be tough if this quality leaves voters unmoved at the polls.

Table 4 details how each leadership characteristic is related to party choice in Australia and Britain when the effects of all the others are controlled. The method used is ordinary least squares (OLS) regression analysis with pairwise deletion of missing data, and the cell entries are unstandardized coefficients.[1] Because the predictor variables are scored 0 or 1, the value for each characteristic can be interpreted as the percentage difference (ignoring the decimal point) in the probability of voting for the prime minister's party between those seeing and not seeing that quality in the leader.

We take the equations that exclude party identification first (cols. 1 and 3 of the table). Since the majority of coefficients are statistically significant, their magnitude is more helpful in determining their

Table 4. The Electoral Effects of Leadership Qualities by Country

Leadership Qualities	Australia		Britain	
Hawke/Thatcher				
Caring	.14*	.04*	.14*	.05*
Determined	.03	.01	.10*	.04*
Shrewd	-.06*	-.03*	.03*	.02*
Likable as a person	.09*	.03*	.05*	-.01
Tough	.05*	.03*	-.01	-.00
Listens to reason	.12*	.03*	.12*	.05*
Decisive	.03	.01	.16*	.06*
Sticks to principles	.08*	.03*	.13*	.05*
Effective	.29*	.13*	.20*	.09*
Howard/Foot				
Caring	-.05*	.01	-.12*	-.05*
Determined	-.06*	-.02	-.05*	-.01
Shrewd	-.02	-.02	.01	.01
Likable as a person	-.01	-.01	-.05*	-.02
Tough	.03	.01	-.02	-.01
Listens to reason	-.10*	-.05*	-.13*	-.04*
Decisive	-.01	.02	-.09*	-.03
Sticks to principles	-.07*	-.04*	-.06*	-.03*
Effective	-.28*	-.13*	-.22*	-.10*
Party identification	—	.63*	—	.71*
R^2	.52	.72	.46	.76

Note: Although grouped separately by leader in the table, all variables were entered into the regression equation simultaneously.

$^*p < .05$.

relative effect on party choice. The principal conclusion is that this effect is far more uniform across leadership characteristics than their varying distribution from leader to leader would have led us to expect.[2] For all four party leaders, their perceived effectiveness is the single strongest influence on voting for their party. The quality of listening to reason also exercises a strong effect across countries as well as leaders. The quality of caring is likewise significant for all four party leaders even if its absolute effect is substantially less powerful for Howard. Sticking to principles is the only other uniformly significant characteristic, but its impact is generally smaller than effectiveness, caring, or listening to reason. These four qualities stand out for the broad uniformity of their impact, but there are one or two others whose influence is more particularized. Most notable among them is toughness. This has a significant effect on the vote only in the case of Hawke. Indeed, it is the only quality that is important only for one leader.

This evidence suggests that contrary to the original hypothesis, leadership appeal in parliamentary democracies is far from idiosyncratic in character. Despite widely varying distributions across individuals, the characteristics that matter for voters seem surprisingly uniform from one country to the other and from one party leader to another. This conclusion could be premature, however, since voters' perceptions of party leaders are to some degree tied up with their party identification.

This is particularly the case for the most influential variable in Table 4, effectiveness. Its mean correlation with partisanship is .46 in Australia and .34 in Britain. We must therefore control for party identification before reaching any firm conclusion about the nature of leadership appeal (idiosyncratic or otherwise) in parliamentary elections.

Repeating this analysis with party identification included leads to the results reported in the second and fourth columns of Table 4.[3] The most obvious result of its inclusion is to bring about a substantial and relatively uniform reduction in the predictive power of the individual leadership qualities. This is only to be expected in view of the close relationship between party identification and party choice in Australia and the same in Britain (zero-order correlations of .82 and .85 respectively). The more pertinent observation, though, is that a good number of leadership qualities nonetheless remain important for how people vote. Indeed, the same four characteristics—effectiveness, listening to reason, caring, and sticking to principles—retain their statistical significance and substantive importance across leaders and countries.

Despite its being the quality most closely associated with partisanship, effectiveness remains the greatest electoral asset for a leader, showing a net effect of at least .09 for all four of them; that is, even taking account of their partisanship, voters who see a party's leader as effective are around 10% more likely to vote for that party than those who do not. Moreover, listening to reason and sticking to principles remain significant for all four leaders, and caring is significant for all but Howard. Thus, 4 of 9 characteristics account for 15 (75%) of the instances of significant relationships in columns 2 and 4 of Table 4. This is not to deny that certain characteristics are personalized rather than uniform in their effect. Determination and decisiveness, for example, are an electoral asset for Thatcher alone, and the same is true of toughness and likability for Hawke. Nonetheless, a perusal of the table indicates that personalized effects are the exception rather than the rule.

A more rigorous examination of the results only serves to reinforce the conclusion of largely uniform effects. For a start, the leadership characteristics make much the same contribution to the overall explanatory power of each equation despite their very different distributions in the two countries. Party identification is slightly more influential in Britain, but the British equation also has a higher R-squared value. By taking the simple correlation between partisanship and the vote (.82 in Australia and .85 in Britain) and subtracting the squared value of each from the appropriate R-squared value in Table 4, leadership qualities can be seen to contribute between four and five percentage points to the explained variance in each country.

Table 5 contains still stronger evidence of the uniformity of leadership effects. It presents a set of t-statistics testing the hypothesis of a significantly different effect for each personal quality from one leader to the next. A separate bilateral test is performed for each pair of the four party leaders.[4] The questions this table addresses specifically are, first, whether the broad impression of uniformity in the electoral impact of leadership characteristics is sustained when tested more systematically and, second, whether (as Table 4 might suggest) this uniformity is truer for some characteristics than others. For prime ministers to be perceived as caring, for example, may make people significantly more likely to vote for the party they lead, but is the strength of its impact much the same in the two countries despite Hawke's far greater likelihood of being credited with this quality (see Table 3)?

Table 5 indicates clearly that the broad answer to both these questions is affirma-

Table 5. T-Tests of Differences between Leaders in the Effect of Each Leadership Quality

Leadership Qualities	Hawke/ Thatcher	Hawke/ Howard	Hawke/ Foot	Thatcher/ Foot	Thatcher/ Howard	Howard/ Foot
Caring	.10	2.41*	.37	.13	2.64*	3.26*
Determined	1.46	.57	.06	1.96*	.96	.54
Shrewd	2.96*	2.55*	1.39	1.99*	.28	1.65
Likable as a person	2.27*	1.26	1.00	1.84	.87	.54
Tough	2.05*	2.15*	1.38	.38	.48	.72
Listens to reason	.60	.32	.24	.54	.08	.33
Decisive	2.62*	1.38	.74	1.34	3.98*	2.07*
Sticks to principles	.89	.11	.27	1.95	.74	.51
Effective	1.54	.08	1.41	.02	1.88	1.79

*$p < .05$.

tive. In the first place, systematic differences across leaders on particular qualities are relatively rare. Less than one quarter of all the t-tests in the table are significant and in no instance are more than three of the six leadership pairings different on any single quality. Furthermore, there are no substantial differences between any pair of the four leaders on a number of characteristics. This uniformity of effect contrasts starkly with the large variation in the distribution of each leadership quality, the extent of which gave early support to the idiosyncracy hypothesis. Distributional differences, in short, bear little relationship to the size of electoral effect; whether a particular quality is seen in a leader by a small or large proportion of voters, that perception is about equally likely to influence the vote. Thatcher, for example, is not generally perceived as being caring; but when seen in her, this quality is as important as it is for Foot, even though he is credited with it by over three times as many people.

In the second place, this uniformity of effect is especially pronounced for the leadership qualities identified in Table 4 as having the strongest influence on voting behavior. Three of these four qualities —effectiveness, sticking to principles, and listening to reason—do not differ, in their impact, between any of the leaders. Indeed, these are the only qualities in Table 5 with this distinction. Moreover, with regard to the fourth quality (caring), only for Howard is its effect different; it does not distinguish Hawke, Thatcher, and Foot from each other. Uniformity, then, is characteristic of the leadership qualities that most influence individual voting behavior, and this conclusion provides powerful evidence against the idiosyncracy hypothesis.

This hypothesis is discredited still further when it is noted that even the concession made to it in Table 4 (i.e., that certain qualities evoke a particularized response at the polls) tends to be invalidated by Table 5. The impact of determination, for instance, is significantly different from zero for Thatcher alone in Table 4. In Table 5, by contrast, it can be seen to differentiate her from Foot but not from Hawke and Howard. Similarly, Hawke's likability is electorally important for him relative to Thatcher, but it fails to set him apart from the other two party leaders.

Distribution versus Effect: The Electoral Consequences

Two strong conclusions follow from the analysis so far. First, the leadership qualities influencing how individuals vote

Table 6. Hypothetical Vote Gains and Losses Attributable to Leadership Qualities

Leadership Qualities	Hawke vs. Howard	Thatcher vs. Foot
Caring	-.05	-1.67
Determined	.28	.72
Shrewd	.80	-.19
Likable as a person	.13	-.36
Tough	-.28	.37
Listens to reason	.26	-.94
Decisive	-.31	1.47
Sticks to principles	-.24	1.32
Effective	3.74	5.85
Total	4.33	6.57

are remarkably uniform from one leader to the next; second, the precise qualities which have an effect are poorly predicted by their distribution among voters. This is not to say, however, that distribution is an inconsequential electoral consideration in parliamentary—or indeed, presidential—elections. For a leader not to be widely credited with one of the electorally important qualities may be just as damaging to his or her party political fortunes as the widespread attribution of it would be advantageous. Thus, this analysis would be incomplete if it failed to recognize that despite being a poor predictor of their impact on individual voting behavior, variation in the distribution of leadership characteristics can have important consequences for the outcome of elections through its influence on the balance of party votes. The effects of individual characteristics may be quite small in themselves, but variation in the extent to which opposing party leaders are seen to possess them can make their combined impact the difference between victory and defeat in a close election.

The importance of distribution in this respect can be illustrated by means of a hypothetical projection of how the 1987 Australian and 1983 British elections would have turned out had the unsuccessful leaders (Howard and Foot respectively) been perceived to possess the range of leadership qualities in the same proportions as their more successful counterparts, Hawke and Thatcher. In other words, that would have happened in the Australian election had Howard been seen as caring, determined, and so on by the same proportion of voters as was Hawke? We can move towards a tentative answer to this question by subtracting the proportion of voters seeing each quality in Howard from the proportion seeing that quality in Hawke (as shown in Table 3). This gives a measure of Hawke's distributional advantage on each characteristic (or disadvantage in the case of sticking to principles). If this figure is then multiplied by the matching Howard regression coefficient (with the sign reversed) from the second column of Table 4, the result is an estimate of the number of percentage points by which the Liberal vote would have increased had the same proportion of voters credited Howard with that quality as credited Hawke with it. In other words, it is a measure of Hawke's net worth (relative to Howard), in votes to the ALP.

The results of this procedure, calculated for both the British and the Australian party leaders, are presented in Table 6. A positive sign indicates that the particular quality is an electoral asset for the prime minister in each country, and a negative sign that it is an asset for the opposition party leader.

Summing the individual coefficients indicates quite clearly that both Hawke and Thatcher were a substantial electoral asset for their party. If Foot had possessed Thatcher's personality profile, Labour's vote in the 1983 general election would, other things being equal, have been some six to seven percentage points higher than it was. The difference is less stark in the Australian case, but the contrast in Hawke and Howard's personality profiles was still worth some four percentage points in the ALP. Even allowing for the statistical error in these estimates, such differences are considerable, and they point up the nontrivial vote gains that can be made by having the "right" party leader, even in parliamentary elections.

Table 6 also highlights the importance of the quality of effectiveness to the definition of the "right" leader. While this quality has much the same effect for all four party leaders, the two opposition parties can be seen to have been seriously disadvantaged by the tendency of voters not to see their leaders as effective. Howard was credited with this quality by 28% fewer Australians than Hawke, while Foot had a massive deficit of 61% relative to Thatcher. After taking account of the tendency of some of the other characteristics to cancel each other out, the combination of the strength of effectiveness' influence on the vote and its highly skewed distribution means that this one quality accounts for a very large part of the hypothetical vote difference in each country. It alone generated an estimated 5.8% vote advantage for the Conservative party in Britain and an estimated 3.7% Labor advantage in Australia. These figures suggest that the perceived difference in the effectiveness of Hawke and Howard was sufficient on its own to tilt the outcome of the 1987 federal election in the ALP's favor.[5]

Conclusion

This analysis has been concerned with specifying the nature of leadership effects in parliamentary elections. Its background is the "presidentialization" of politics in this type of political system and the barrage of adverse criticism that has greeted this development. The importance of leaders is not necessarily new. It is quite probable that a number of leaders in the past, such as Menzies in Australia and Churchill in Britain, also symbolized their parties in the public eye. But one obvious and crucial difference between then and now is television and the central role it has come to play in political campaigning and communication. Critics argue that the personalization of the election campaign strategies adopted by political parties, particularly through this medium, trivializes democracy as individuals are encouraged to make their voting decisions on the basis of ill-informed judgments about the idiosyncratic personality characteristics of individuals who rise and fall from the political stage. In the words of one respected Australian journalist: "We are now firmly in the age of polished, packaged politics; image is on a par with intellect; presentation matters as much or more than ideas. . . . The contestants . . . trade simplified, if not simplistic, slogans that will indicate at only the most generalized level the values and priorities on which they differ. . . . The tailor, hairdresser and color consultant will become important political advisors."[6]

Our analysis speaks directly to this debate in two respects. In the first place, it confirms that just as with candidates in presidential elections, party leaders can influence the distribution of votes in parliamentary contests. Indeed, Hawke would seem to have been the difference between victory and defeat for his party in 1987 (see also Bean and Kelley 1988). While the same cannot be said for Thatcher in 1983, dealignment and the rise of minor parties in Britain could mean that party leaders will find themselves playing the same pivotal role there on occasion.

In the second place, little support is found for the claim that leadership characteristics affecting the way people vote are idiosyncratic and trivial. We made every effort to ensure that the idiosyncratic prevailed over the systematic in this examination of leadership effects in two countries. The evidence, however, points clearly to the unimportance, by and large, of the idiosyncratic, suggesting instead that prime ministerial candidates are judged against some kind of well-defined schema in the public mind and that they will have a positive electoral impact to the extent that they conform to this mental image of what a leader should be like. Miller, Wattenberg, and Malanchuk (1986) demonstrate this purposive evaluation of political leaders to hold true over time in U.S. presidential elections; we demonstrate it to hold true across countries and leaders in parliamentary elections.

Nor is there much reason to believe that it is the trivial in leaders that appeals to voters. If it were, we would expect the highly personal quality of likability in particular to have had a strong—if not the strongest—effect on voters. As it turns out, however, it is of little importance overall. Instead, perceived effectiveness dominates how voters respond to leaders, and few would deny that this quality is a key ingredient of successful political leadership, especially when viewed from the parliamentary perspective of implementing the manifesto on which the government was elected. Their ability to have demonstrated their effectiveness in office means that other things being equal, incumbent prime ministers go into an election with an advantage over opposition party leaders who have not held the top job before. This advantage is limited, though, since voters who perceive the opposition party leader to be effective are no less likely to vote for his party than are those who see the same quality in the incumbent prime minister. For this apparent incumbency advantage to be neutralized, in other words, the calculus of voting does not have to be reshaped. The challenger faces the less onerous task of matching the incumbent in conveying the image of being likely to be an effective prime minister.

These results indicate, above all, a pressing need to understand better the nature of leadership appeal in democratic political systems, be they parliamentary or presidential. It is not enough to know, for example, that Australians and Britons are particularly likely to be swayed in their voting decision by the perception of leadership effectiveness. There is also the question of what this quality means for voters and whether its meaning is the same from one country to another.

Appendix

Data Sources

The Australian data come from the 1987 Australian Election Study directed by Roger Jones, Ian McAllister, and Anthony Mughan, which is available from the Social Science Data Archives at the Australian National University. It is a nationwide, self-completion mail survey comprising an original sample of 3,061 adult Australians drawn randomly from the June 1987 electoral rolls. The first questionnaires were sent out two days before election day (11 July 1987). A reminder-thank you postcard was then mailed to all sample members on 17 July. A second questionnaire and covering letter were mailed to the remaining nonrespondents on 3 August and a final reminder letter was sent on 20–21 August to those who had still not replied. The overall response rate was 62.8%. The British 1983 Election Study was conducted by Anthony Heath, Roger Jowell, and John Curtice and was supplied by the Economic and Social Research Council Data Archive at the University of Essex. It is a face-to-face interview survey whose original representative sample comprised 6,000 voters. Two hundred fifty constituencies were selected with probability proportionate to size of electorate. One polling district within each constituency was then selected and a random sample of twenty four electors drawn with equal probability from it. After a further attempt to locate or persuade potential respondents, a response rate of 72.4% was achieved. Fuller methodological details of the data collection can be found in McAllister and Mughan 1987 and Heath, Jowell, and Curtice 1985 respectively. The authors of this article, of course, bear sole responsibility for the analyses and interpretations contained in it.

The Leadership Effectiveness Question

Both surveys asked the question, "On the whole, how effective or ineffective do you think Mr. Hawke [Mrs. Thatcher] is as a prime minister?" This was followed by "And how effective or ineffective do you think Mr. Howard [Mr. Foot] would be as a prime minister?" British respondents, however, were asked to choose between *very effective, fairly effective, fairly ineffective,* and *very ineffective* in response to the question. For comparability with the other leadership qualities, these categories were recoded to a dichotomy with *effective* being scored 1 and *ineffective* scored 0. Australian respondents, on the other hand, were presented with a fifth, intermediate response category, *it depends.* This category attracted many respondents (16% for Hawke and 28% for Howard) and was scored .5 in preference to eliminating these respondents from the analysis. The substantive conclusions drawn would have been unchanged had these respondents been eliminated.

The Zero-Sum Methodology Issue

This issue concerns the effect of the analytical methodology adopted. The substantive conclusion of a broadly uniform leadership effect across countries would be the same had we used a zero-sum methodology in which a score of 1 is given to the incumbent prime minister if he or she is seen as possessing a particular quality and the opposition party leader is not, a score of -1 is given when a quality is attributed to the opposition party leader and not to the prime minister, and a score of 0 is given when both *or* neither is seen as having the quality in question. The methodology described in the text is preferred because its coding scheme allows us to focus on individual leaders as well as countries. This dual focus makes for firmer and more confident conclusions about the nature of leadership effects generally.

Notes

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1. We also estimated the equations using ordered logit and ordered probit regression models (Maddala 1983). Both produced results that leave our substantive conclusions unaltered, reaffirming that in practice OLS regression is an appropriate technique to use in this type of analysis (see, e.g., Kelley and McAllister 1985; Powell 1986). The OLS estimates are used in the presentation of our findings since they are more easily interpreted than logit or probit coefficients.

2. We also tested in various ways for the possibility that the leaders of significant minor parties—Ian Sinclair of the National party in Australia and David Steel and Roy Jenkins of the British Liberal and Social Democratic parties respectively—should properly be included in the analysis. All the indications were, however, that these leaders are trivial electoral influences and that the significant contest was between the prime minister and the leader of the opposition.

3. Initially, a wide range of social structural variables were used as controls in addition to party identification. Their inclusion was found to be superflous, however, and they were dropped from further consideration for the sake of simplicity and clarity in the presentation of the results.

4. Different procedures are required to test for significant differences within each of the two samples on the one hand and across them on the other. See Johnston 1984 (pp. 338–41) for the former and Jencks et al. 1979 (p. 39) for the latter. In executing these tests, the signs of the coefficients were adjusted to take account of the fact that we wished to test whether the strength of the effects differ or not, given that, by and large, their direction for each leader favors the leader's own party (with occasional exceptions to this rule, which the tests account for).

5. Other things being equal, a further uniform movement of 3.7% to the Liberal-National parties would have given them a majority of at least 10 seats instead of the 24-seat majority actually won by the ALP. See Malcolm Mackerras, "How Labor Made Most of the Least," *Bulletin*, September 1987.

6. Geoffrey Barker, "Television Blood Sport and the Future of Democracy," *Melbourne Age*, 20 October 1984.

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BARGAINING IN LEGISLATURES

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Bargaining in legislatures is conducted according to formal rules specifying who may make proposals and how they will be decided. Legislative outcomes depend on those rules and on the structure of the legislature. Although the social choice literature provides theories about voting equilibria, it does not endogenize the formation of the agenda on which the voting is based and rarely takes into account the institutional structure found in legislatures. In our theory members of the legislature act noncooperatively in choosing strategies to serve their own districts, explicitly taking into account the strategies members adopt in response to the sequential nature of proposal making and voting. The model permits the characterization of a legislative equilibrium reflecting the structure of the legislature and also allows consideration of the choice of elements of that structure in a context in which the standard, institution-free model of social choice theory yields no equilibrium.

Lasswell (1968) suggested that the fundamental question of politics is how the gains from a bargaining situation are to be distributed among members who have differing and sometimes conflicting preferences. Bargaining in legislatures reflects the structure of the legislature and the formal rules that govern agenda formation and voting. Shepsle (1979) models this structure and characterizes an equilibrium based on a concept of stability drawn from the field of social choice. We take a strategic approach to bargaining and study a stylized model of distributive of expenditure policy in a unicameral, majority rule legislature not favoring any member of the legislature or any particular outcome. For this distributive problem, Arrow's (1951) impossibility theorem implies that in the framework of social choice theory there is no voting equilibrium. The absence of an equilibrium is a consequence of the opportunity to pit simultaneously and costlessly any alternative against every other alternative. In contrast to this institutionless setting, the theory presented here reflects the sequential nature of proposal making, amending, and voting, and models it as a noncooperative, multisession game. Noncooperative equilibrium can be shown to exist, and the legislative outcomes reflect the institutional structure of both the agenda formation process and the voting mechanism. Indeed, if the legislature is not too small, any distribution of the benefits can be supported as an equilibrium; that is, where social choice theory finds no equilibrium, every distribution is an equilibrium when the sequential structure of the legislative process is taken into account. These equilibria, however, are supported by complex punishment strategies that are unlikely to be self-enforcing in a large legislature with turnover. We thus focus on stationary equilibria, which have a natural focal point property.

One issue to be investigated is whether

the legislative equilibrium reflects the majoritarian nature of the voting rule, as predicted, for example, by Riker's (1962) theory of minimal winning coalitions or whether benefits are distributed universally, as predicted by Weingast (1979). As might be expected, the answer depends on the amendment rule employed. Under a closed rule a majoritarian outcome results, and within the majority the author of the winning proposal receives between half and two-thirds of the benefits. Under an open rule, the outcome can be universalistic if the legislature is small and the members find delay in reaching an outcome to be very costly. In large legislatures, however, majoritarian outcomes result under an open rule, but the distribution among the winning majority is more equal than under a closed rule. An open rule can also result in delay in reaching a resolution, whereas the legislature completes its task in the first session under a closed rule. With respect to the choice of an amendment rule by the legislature, if delay is costly to the members of the legislature, the members will unanimously prefer that the legislature operate under a closed rule.

Since ours is a multilateral bargaining model, it is useful to contrast our results with those obtained in bilateral models. Rubinstein (1982) and Binmore (1986) find a unique equilibrium in bilateral bargaining models in which the first proposal is accepted when information is complete, players are impatient, and the rules governing who may make and accept offers are exogenous.[1] Legislative choice differs significantly from bilateral exchange in several respects. First, bilateral exchange requires unanimous consent for an outcome, and this requirement gives each party veto power that is reflected in the equilibrium outcomes. In a majority rule legislature, no member possesses veto power. Second, in bilateral bargaining, if agents are identical and make alternating offers, equilibrium distributions approach equality as impatience diminishes, as Rubinstein (1982) showed.[2] In the legislative model considered here, majority rule equilibria do not generally tend to equal distributions, and equilibria in which some members receive nothing may occur even as impatience goes to zero. Third, in bilateral and multilateral bargaining with complete information and a unanimity rule, the equilibria are such that the first offer is always accepted. In a majority rule legislature in which amendments can be made, the first offer is rejected with positive probability in a stationary equilibrium when impatience is small. Furthermore, the probability is positive that the legislature will not reach an outcome in a finite number of sessions.

Endogenous agenda formation in legislatures has recently received renewed attention. Miller (1986) analyzes a specialized model with two competing agenda setters, but his model does not capture important features of real legislatures. Ferejohn, Fiorina, and McKelvey (1987) consider a model in which agendas are formed by members of a voting body proposing alternatives freely and voting only when an equilibrium agenda has been formed. They show that with certain special restrictions on the set of alternatives, common institutional rules about proposal making yield agenda equilibria and unique voting outcomes, but generalizations within this framework seem difficult to achieve. Banks and Gasmi (1987) consider a model of endogenous agenda formation in a legislature with three members who have Euclidean preferences and may make one or two amendments to the proposal made by the member initially recognized. Their model is sequential, as is the model considered here; but their legislature has only one session and thus differs from the sequential bargaining framework employed here. Harrington (1986) studies an *n*-member legislature in which a member is chosen at random to make a proposal that must be accepted or rejected by an α-majority. The legislature meets for a finite number of sessions and members are not impatient. The structure of Harrington's model is similar to that considered here in the "Illustration" below, although his focus is on the effects on the distribution of risk aversion and of the required majority. Finally, Epple and Riordan (1987) consider a model in which a sequence of distributions is to be chosen. They employ a simplified institutional structure that resembles that studied in the third and fourth sections here, and their interest is in demonstrating that all allocations can be supported as subgame-perfect equilibria.

The model of the legislature is introduced along with an example that illustrates the equilibrium concept and the influence of legislative structure. We then show that with sufficiently little impatience on the part of members, any outcome can be supported as a subgame perfect equilibrium. Stationary equilibria are discussed, and a stationary equilibrium is characterized for a legislature governed by a closed rule that prohibits amendments, then for a legislature governed by an open rule that permits amendments. We then consider the ex ante choice of an amendment rule and offer conclusions.

The Model of a Legislature

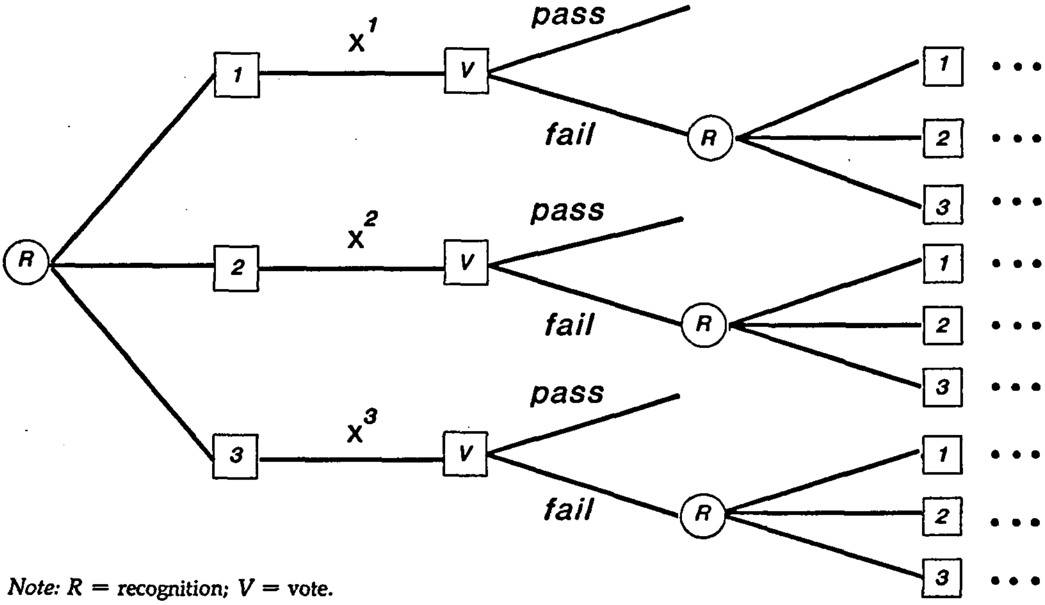
Legislative Structure

The model is intended to reflect in a stylized manner the endogenous nature of proposal making and voting, and to allow a comparison between amendment rules used to govern when an amendment can be made and when a proposal is brought to a vote. The legislature consists of (1) n members, each of whom represents a legislative district, (2) a recognition rule that determines which member may make a proposal, (3) an amendment rule, and (4) a voting rule. The "members" could be either individuals or unified blocks of legislators who have the same preferences.

The task before the legislature is to choose a nonnegative distribution of one unit of benefits among the districts according to majority rule, with no side payments outside the legislature permitted. The task of the legislature in the model considered here thus corresponds to situations in which benefits can be distributed among districts in any manner at the discretion of the legislature. Each member is assumed to have risk-neutral preferences that depend only on the benefits distributed to the member's district, so the legislative task is one in which members have conflicting preferences and in which there is no majority rule equilibrium in the standard social choice framework. Preferences and the legislative rules are assumed to be common knowledge, and all actions are observable; so the model involves perfect information.

The legislature is governed by a recognition rule that identifies a member who is recognized to make a proposal or to bring the legislature to a vote. Since recognition is valuable in equilibrium, every member will attempt to be recognized; thus the legislature must have a rule for deciding who shall be recognized. In order not to bias the results in favor of any member or in favor of a particular amendment rule, the recognition rule is chosen to be neutral, which requires that it select a member at random to make a proposal.[3] Thus at the beginning of a legislative session, member i has a probability p_i of being recognized, and if recognized, make a proposal that specifies how the benefits are to be distributed. This proposal is then the motion on the floor. A proposal is a distribution $x^i = (x_1^i, \ldots, x_n^i)$, such that $\Sigma_{j=1}^n x_j^i \leq 1$, so the set X of feasible proposals is an n-dimensional simplex. The status quo corresponds to no allocation of the benefits.

Figure 1. Legislative Process: Closed Rule



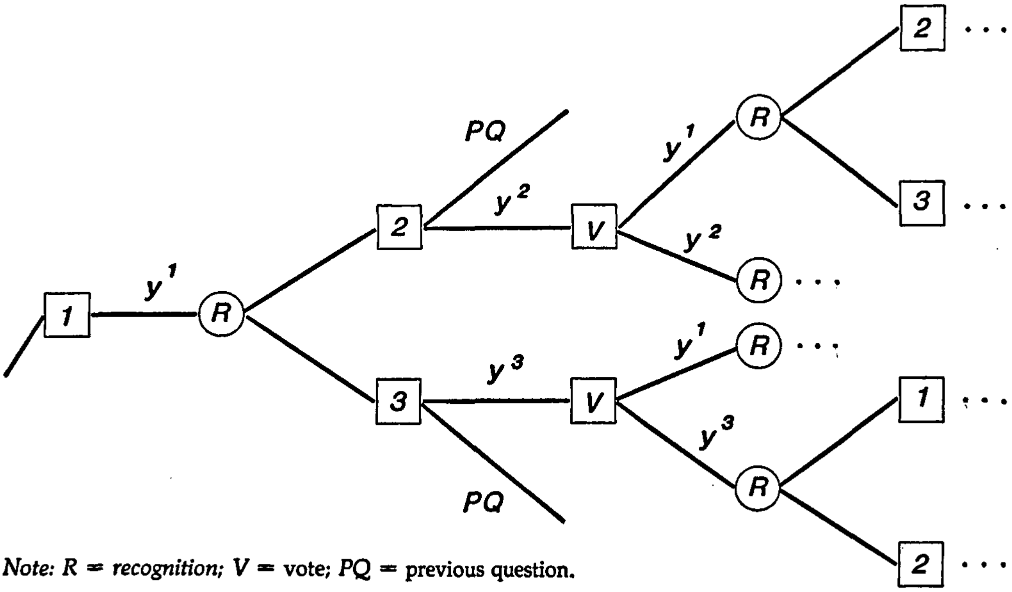
Note: R = recognition; *V* = vote.

The resolution of the motion on the floor depends on the amendment rule employed by the legislature. Under a *closed rule* the motion is voted on immediately (against the status quo); and if it is approved, the legislative adjourns. If the motion fails, the status quo prevails with the benefits remaining unallocated. The legislature has no means of committing not to consider distributing the benefits in the next session. Thus, when the legislature moves to the next session, the process repeats, with a member recognized to make another proposal, and so on. The legislative process under a closed rule is illustrated in Figure 1.

Under an *open rule* amendments can be offered to the motion on the floor, where an amendment is in the nature of a substitute and hence is an element of X;[4] that is, in the same session another member j ($j \neq i$) may be recognized with probability $p_j/\Sigma_{k \neq i} p_k$ and may either offer an amendment to the proposal or move the previous question. The term *previous question* is used here to indicate that the amending process is concluded and a vote is required on the motion on the floor. This differs somewhat from congressional usage and perhaps corresponds most closely to the House's rising from the Committee of the Whole to vote on amendments and bills.[5] If the previous question is approved, the legislature votes on whether to accept the proposed distribution of the benefits. Because a member would never move the previous question unless he or she anticipated that the motion on the floor would be adopted, the vote on the previous question need not be incorporated in the model. Thus, moving the previous question is equivalent to bringing the motion on the floor to an immediate vote. If that motion is approved, the legislature adjourns.

If the member recognized does not move the previous question, the member offers an amendment, which is itself a

Figure 2. Legislative Process: Simple Open Rule



Note: R = *recognition; V* = vote; *PQ* = previous question.

proposed distribution of the benefits and may be viewed as a substitute for the motion on the floor. To facilitate the analysis, a simple open rule will be considered allowing no more than one amendment to be on the floor at any time;[6] that is, once an amendment is offered, it must be disposed of by bringing it to an immediate vote against the proposal on the floor.[7] Given that amendments are in the nature of a substitute, such a restriction seems natural. If the amendment fails, the proposal remains on the floor, and the next session commences. If the amendment obtains a majority, the amendment becomes the proposal on the floor, and the legislature continues to the next session. At this point, a member is recognized who may offer another amendment or may move the previous question on the proposal on the floor. The process continues with members proposing amendments or moving the previous question until the proposal or one of the amendments has defeated the motion, if any, on the floor and has been passed upon movement of the previous question. The structure of the legislative process under an open rule is illustrated in Figure 2.

In contrast to models in which the agenda is exogenously imposed, members in the legislature considered here must form expectations as to which future proposals will be made and how members will vote on them. If a member fails to vote for the proposal on the floor, that member runs the risk that in the next session a proposal could be passed allocating no benefits to his or her district. This provides an incentive for the member to vote for the proposal on the floor if it provides an allocation to the member's district at least as great as can be expected from future legislative sessions. Because the legislature may not distribute the benefits in the first session, the time preference of members may play a role in legislative behavior. Members are thus assumed to have a common discount factor $\delta \leq 1$, which may reflect the political imperative resulting from reelection concerns to distribute the benefits sooner rather than later. The parameter may also represent the probability that the member will be returned to office in the next election; when δ is interpreted in this manner, the member is assumed to have preferences only for the benefits he or she delivers to the district. The preferences of member j are thus represented by the utility function $u^j(x^k, t) = \delta^t x_j^k$, where t is the session in which the legislature adopts the distribution x^k.

The process of proposal generation and voting yields an extensive form game with an infinite game tree. A *history* h_t of the game up to time t is a specification of who had a move at each time, the move selected by each member every time a move was allowed, and the vote when a vote was required. A pure strategy s_τ^i at time τ is a prescription of what motion to make when a member is recognized and of how to vote whenever a vote is required; that is, if H_τ denotes the set of histories, a pure strategy is $s_\tau^i : H_\tau \rightarrow X$ if τ is the beginning of a session and i is recognized and is $s_\tau^i : H_\tau \rightarrow \{\text{yes, no}\}$ if τ is a time to vote, where {yes, no} are the two voting alternatives. A *strategy* s^i of member i is a sequence of functions s_τ^i mapping H_τ into that member's available actions at time τ. A randomized strategy σ_τ^i at time τ is a probability distribution over the strategies s_τ^i available at time τ. An important feature of this model is that whenever a member is to take an action, he or she knows which history has occurred; so the game is one of perfect information.

Members are not able to make binding commitments either to vote in a particular manner or to offer a particular proposal. Thus, an equilibrium strategy must be self-enforcing in the sense that the member would wish to execute it at each point in the game at which an action can be taken. Therefore, the equilibrium is required to be subgame-perfect. An equilibrium configuration of strategies is subgame-perfect if the restriction of those strategies to any subgame constitutes a Nash equilibrium in that subgame. For any particular subgame-perfect equilibrium, the *value* $v(t, g)$ of subgame g after t sessions is defined as the vector of values $v_i(t, g)$, with $i = 1, \ldots, n$, to the members that results from the play of that subgame-perfect equilibrium strategy configuration. The *continuation value* $\delta v_i(t, g)$ is the value if the legislature moves to subgame g. The ex ante value at the beginning of the game is denoted by v_i, $i = 1, \ldots, n$.

Whenever a vote is required, voting takes place sequentially and in public; that is, there is a fixed order in which each member must announce how he or she will vote on the question before the body and every other member may observe each vote as it is cast, so members always know the entire history of play whenever they must take an action. Because of the "coarseness" of voting rules—the fact that if a proposal wins by more than one vote no member can change the outcome by changing this vote—we restrict attention to a refinement of the subgame-perfect equilibria, in which weakly dominated strategies are eliminated. Because votes and proposals occur sequentially and openly, the weakly undominated subgame-perfect equilibria correspond to dominant-solvable solutions.[8] (For simplicity's sake, we shall no longer explicitly refer to the elimination of weakly dominated strategies.)

An Illustration: A Closed Rule and a Finite Number of Sessions

To illustrate the basic structure of the model, consider the simple case of an n-member, two-session legislature governed by majority rule, with equal probabilities of recognition and in which a closed rule prohibits amendments once a proposal has been made. In this setting, if by the end of the second session no bill has been passed, the legislature dissolves, and each member receives zero. This might correspond to the case of benefits that expire after a fixed time.

A history in this case is simply a description of who was recognized, what proposal was made, and how each member voted for each session. A strategy for a member specifies the bill to offer when recognized and the set of bills to vote for when required to vote. A member will be assumed to vote for a bill if indifferent between the distribution in the bill and the continuation value of the subgame beginning in the next session.[9] Similarly, if a member knows that his or her vote will not be decisive (because, e.g., a majority will vote for or against the bill), the member is assumed to vote for the bill if and only if the member's distribution in the bill is at least as great as the member's continuation value.

Unlike the more general case considered later, the extensive form of this game is finite, which permits backwards induction to be used to derive the equilibrium. In such a legislature, there is a subgame-perfect equilibrium in which each member, if recognized, makes the same "canonical" majoritarian proposal to distribute the benefits to a minimal winning majority. The equilibrium strategies and distributions are thus unique up to the specification of who receives the benefits.

PROPOSITION 1. *A strategy configuration is a subgame-perfect equilibrium for a two-session, n-member (with n odd) legislature with a closed rule and equal probabilities of recognition if and only if it has the following form:*

a. If recognized in the first session, a member makes a proposal to distribute δ/n to any $(n - 1)/2$ other members and to keep $1 - \delta(n - 1)/2n$ for his or her district. If recognized in the second session, a member proposes to keep all the benefits.

b. Each member votes for any first-session proposal in which the member receives at least δ/n and votes for any second-session proposal.

The first proposal is thus accepted, and the legislature adjourns in the first session.

Proof. To establish necessity, note that since the game ends after the second session, the continuation value $v_i(2, g) = 0$ for all (null) subgames g and for all members. The member recognized at the beginning of session 2 can thus successfully propose to take all the benefits, since each member will vote for it because the member's allocation is at least as great as the member's continuation value. The continuation value $v_i(1, g) = 1/n$ for any second-session subgame g is the expectation of member i's distribution contingent on who is recognized. Thus, in the first session, a member will vote for a proposal only if the member receives at least δ/n, the value of defeating a proposal and continuing to the second session. Any member recognized at the beginning of the first session will thus offer δ/n to a minimal number $(n - 1)/2$ of other members needed to obtain a majority vote. Recognized thus, the member keeps $1 - \delta(n - 1)/2n$. The $(n - 1)/2$ other members and the proposer vote for the proposal, and the other members vote against it.

Sufficiency follows from direct verification that strategies satisfying conditions a and b constitute a subgame-perfect equilibrium, using the fact that before anyone is recognized, the continuation value $v_i(1, g) = 1/n$ for any subgame g. QED.

The equilibrium in proposition 1 follows from five features of the model: (1) subgame perfection, (2) a finite legislature, (3) random recognition, (4) a closed rule, and (5) majority rule. Subgame perfection and finiteness are used to identify the equilibrium strategies in the second session, and random recognition and subgame perfection determine for each member the continuation values of the subgames. The closed rule prohibits others from making counterproposals or amendments to the proposal on the floor. Majority rule implies that in the first session the member recognized need only obtain the votes of $(n - 1)/2$ other members, and those votes can be obtained by offering each a distribution equal to the member's continuation value.

Several important features are evident in this equilibrium. First, the distribution of the benefits reflects the majoritarian distribution of power in the legislature in that only a minimal majority of members receives a positive allocation of benefits. Second, the member recognized in the first session receives the largest allocation. The power of the member recognized first results both from agenda power and from the closed rule that prohibits amendments. Agenda power results from recognition which gives the proposer an opportunity to choose strategically a proposal for which a majority of members will vote. The closed rule prohibits other members from challenging the motion on the floor with an amendment, so those who in the motion on the floor will receive a distribution less than their continuation value can respond only by voting against the motion and, if they constitute a majority, continuing to the next session.[10] To indicate the extent of this power, note that the distribution received by the proposer can be as large as $[1 - (\delta/3)]$ in a three-member legislature, and the limit as n becomes arbitrarily large is $[1 - (\delta/2)]$. Thus, the benefits received by the member recognized are in the interval $[1 - (\delta/2), 1 - (\delta/3)]$, so the member recognized always receives at least half the benefits.

Third, the initial offer is accepted and the legislature adjourns after only one bill has been proposed and voted on. This results from the incentives created by majority rule rather than from impatience as in the bargaining models of Rubinstein (1982) and Binmore (1986). Majority rule —or more generally any rule that does not require unanimity among all the members—and a closed rule allow the proposer to form a majority to distribute the benefits among themselves to the exclusion of the other members. The member recognized need distribute only the continuation value to $(n - 1)/2$ other members, and the opportunity to keep the rest leads the member recognized to make a proposal that will be accepted. The fact that the legislature adjourns after the first session is thus not due to impatience but rather to majority rule and to the inability of other members to amend the proposal of the member recognized. As will be demonstrated, if amendments are governed by an open rule, the benefits are distributed more evenly and the legislature may not complete its task in the first session.

Although the equilibrium in proposition 1 distributes the benefits among a minimal majority of the legislature, this minimal majority is not a coalition in the sense the term is used in cooperative game theory or in the social choice literature. The members of the majority in the legislature considered here act noncooperatively, and find it in their own individual interests to act as specified in the equilibrium. Even though the equilibrium has the property that the benefits are distributed among a minimal majority of members, this conclusion differs in two ways from Riker's (1962) theory of minimal winning coalitions. First, Riker's theory is based on the theory of cooperative games, and the theory developed here is noncooperative. Second, the noncooperative theory provides predictions about the distribution of the benefits among the members of the majority.

The result in proposition 1 is directly generalizable in a number of directions. First, the result can be extended to any finite number of sessions. Second, in a two-session legislature if the members have different probabilities p_i of being recognized, each has a continuation value $v_i(1, g) = p_i$ for any second-session subgame. Then, if member k is recognized in the first stage, he or she can offer δp_i to the ith member and that member will vote for the proposal. Member k will thus choose the $(n - 1)/2$ members with the lowest p_i. Note that depending on the probabilities the member with the lowest probability of recognition may have the highest ex ante value of the game, and the member with the highest probability of recognition may have the lowest ex ante value of the game.[11] For example, if $n = 3$, and $p_2 = 1/3$, $p_1 = 1/3 + \epsilon$, and $p_3 = 1/3 - \epsilon$, $\epsilon > 0$, the ex ante values v_i of the game have limits $v_1 = 2/9$, $v_2 = 1/3$, $v_3 = 4/9$ as $\epsilon \rightarrow 0$. The member with the lowest probability of recognition thus can do better than the other members because he or she is a less costly member of any majority.

There are a continuum of subgame-perfect, Nash equilibria satisfying the conditions of proposition 1. For example, in the first session member i may make a proposal that distributes δ/n to any of the $(n - 1)/2$ other members. One equilibrium thus has the member recognized randomizing among the other members resulting in an ex ante value $v_i = 1/n$ for each member. This equilibrium has the property that the continuation values for all subgames are the same, or stationary, and equal to $1/n$. This stationary equilibrium is also symmetric in both the continuation values and the strategies. As demonstrated later, in an infinite-session legislative game with δ sufficiently high, any distribution of the benefits can be supported as a subgame-perfect, Nash equilibrium. Later, stationary equilibria are argued to possess a focal point property.

Equilibria in an Infinite-Session Legislature with a Closed Rule

In the finite-session legislative game just considered, backward induction could be used to characterize the equilibria. When the number of sessions is unlimited, however, any distribution of the benefits may be supported as a subgame-perfect equilibrium if there is a sufficient number of members and if they are not too impatient. In contrast to the uniqueness result for two-member bargaining, multimember bargaining under majority rule allows virtually all distributions of the benefits to be supported as equilibria.[12]

PROPOSITION 2. *For an n-member, majority-rule legislature with an infinite number of sessions and a closed rule, if* $1 > \delta > (n + 2)/[2(n - 1)]$ *and* $n \geq 5$, *any distribution x of the benefits may be supported as a subgame-perfect equilibrium. To support an arbitrary distribution* $x \in X$, *employ the following strategy configuration: (1) whenever a member is recognized, he or she is to propose x, everyone is to vote for x; (2) if a majority rejects x, the next member recognized is to propose x; (3) if a member j is recognized and deviates by proposing* $y \neq x$,

a. a majority M(y) is to reject y

b. the member k recognized next is to propose z(y) such that for the deviator $z_j(y) = 0$, *and everyone in M(y) is to vote for z(y) over y;*

(4) if in strategy 3b member k is recognized and proposes $s \neq z(y)$, *repeat strategy 3 with s replacing y and k replacing j.*

The proof is presented in the Appendix. The idea of the proof is simple: the required strategy configurations are such that any member who deviates from the prescribed distribution or from the prescribed punishment is certain to be punished, since subgame strategies constitute perfect equilibria. Members expect the punishment to be enforced because they expect that anyone who fails to punish a deviator will in turn be punished, and so on. When punishments of this sort are available, any outcome can be supported as a subgame-perfect equilibrium for $n \geq 5$ and a sufficiently high δ. Consequently, any vector of ex ante values of the game to the members may be supportable by a subgame-perfect equilibrium.

It should be clear that the degree of impatience plays very little role in the proof. As long as members do not discount the future too much, any distribution may be supported. For very small legislatures, this restriction is consequential, but as n increases, the required δ declines rapidly to 1/2. If there is no impatience, however, a member may find a deviation attractive, since under the recognition rule there is a positive probability that that member will be the one recognized in each session. If the member fails to punish him- or herself and $\delta < 1$, the value from such deviations is zero in an infinite session legislature, so deviation does not pay. If $\delta = 1$, however, that value does not go to zero. Under an alternative recognition rule in which no member may be recognized in consecutive sessions, punishment of a deviation is certain, and the result of proposition 2 holds for $\delta = 1$ and $n \geq 5$.

In contrast to the theory of social choice, which finds there is no equilibrium for this distribution problem, the theory presented here predicts that all distributions may be achieved as noncooperative equilibria in a model that captures in a simple manner the sequential nature of proposal making and voting.

Stationary Equilibria

In view of proposition 2, a theory of agenda formation that yields sharp predictions must be based on some refinement of the set of equilibria. The refinement considered here locates a "focal" equilibrium as discussed by Schelling (1960). We argue that the equilibria based on the punishment strategies of the sort displayed in proposition 2 are not "natural" focal points, since each member believes that if there is a deviation all the other members will carry out their designated punishment strategies at each of the infinitely many times they might be recognized. But if the game were to be played, a potential deviator would know that at some point some member would be required to make a punishment proposal in a circumstance in which that member is actually indifferent to doing so. Such a member may renege without cost, so a potential deviator may come to suspect that some such member in some session would renege with positive probability. Believing this, he or she would assign probability less than one to the event that all punishments would be carried out and would, moreover, know that everyone else would share this skepticism. In this sense, the punishment strategies that support any distribution x are not robust: if members are indifferent as to whether or not they punish deviators, the required strategies are only weakly credible.

Indeed, if δ is interpreted as a reelection probability, nonstationary strategies of the sort required for proposition 2 hardly seem plausible.[13] Unless new members are assumed to be able to know the whole history of play when they enter and to find it in their interests to act on that history, strategies conditioned on that history may be impossible to implement. Furthermore, with new members entering each period, sitting members might be expected to have less reason to believe that punishment strategies would be implemented.

From a different perspective, note that in equilibrium the punishment strategies are never actually used, since the first proposal is x and it is approved. If there is a cost to devising and being prepared to employ complex strategies of the type needed to support the equilibrium in proposition 2, the set of equilibria may be much smaller. In the context of a repeated game, Abreu and Rubinstein (1988) have provided such a theory.

The fragile nature of the equilibria in proposition 2 thus suggests that infinitely nested punishment strategies might not actually be played in legislatures because members would be unwilling to rest their fates on such weak incentives. Instead, members might find it natural to restrict their behavior in a way that calls for the same strategy in structurally equivalent subgames.[14] Two subgames are *structurally equivalent* if (1) the extant agenda at the initial nodes of the subgames are identical, (2) the sets of members who may be recognized at the next recognition node are the same, and (3) the strategy sets of members are identical. With a closed rule, two subgames commencing with the null agenda (i.e., no motion on the floor) are thus structurally equivalent, so all subgames commencing after the defeat of the proposal on the floor are structurally equivalent. A subgame commencing with a bill x on the agenda for a vote and a subgame commencing with a bill y on the agenda for a vote are not structurally equivalent if $x \neq y$ even though the strategy sets of the members are the same.

An equilibrium is said to be *stationary* if the continuation values for each structurally equivalent subgame are the same. A stationary equilibrium necessarily has strategies that are stationary; that is, they dictate that a member take the same action in structurally equivalent subgames. Thus, if a member is recognized when there are no motions pending at each of two different sessions, he or she makes the same proposal in both sessions. Stationary strategies are necessarily history-independent and thus are chosen in response only to the incentives presented by the play in future sessions.[15] Clearly, the strategies in proposition 2 fail to satisfy this restriction, since what a member is required to propose depends on the history of play leading to the subgame.

A Closed Rule and Stationary Equilibria

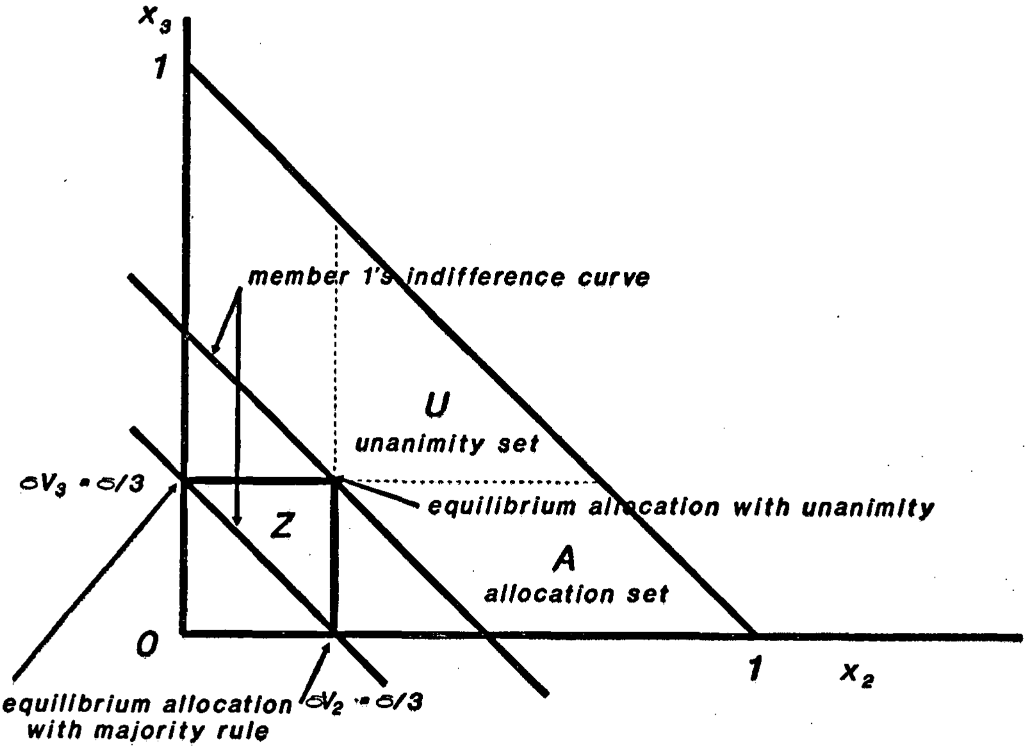
A Legislature with an Infinite Number of Sessions

The results of the "Illustration" indicate that with a finite number of sessions (1) the equilibrium distributions of benefits are majoritarian, (2) the member recognized has power in the sense that his or her allocation of benefits is strictly greater than that of any other member, and (3) the legislature completes its task in the first session. We now investigate an equilibrium with the stationarity property to determine whether the qualitative properties of the equilibria in the finite session case also hold when the number of sessions that the legislature can meet is not arbitrarily limited.

Initially, we treat only the symmetric case in which member i has a probability $p_i = 1/n$ of being recognized, then the case of unequal probabilities. The following proposition indicates that the qualitative properties of the finite session case are preserved in a stationary equilibrium.

PROPOSITION 3. *For all $\delta \in [0, 1]$ a configuration of pure strategies is a stationary subgame-perfect equiibrium in an infinite session, majority rule, n-member (with n odd) legislature governed by a closed rule if and only if it has the following form: (1) a member recognized proposes to receive $1 - \delta(n-1)/2n$ and offers δ/n to $(n-1)/2$ other members selected at random; (2) each member votes for any proposal in which at least δ/n is received.*[16] *The first proposal receives a majority vote,*

Figure 3. Closed Rule and Stationary Strategies


x_3
1
member 1's indifference curve
U
unanimity set
$\delta v_3 = \delta/3$
Z
equilibrium allocation with unanimity
A
allocation set
0
1
x_2
equilibrium allocation with majority rule
$\delta v_2 = \delta/3$

so the legislature completes its task in the first session. The ex ante values of the game are $v_i = 1/n, i = 1, \ldots, n$.

The proof is presented in the Appendix. The role of majority rule in proposition 3 can be identified from Figure 3 for the case of $n = 3$. The recognition rule and the requirement that strategies satisfy subgame perfection yield stationary values (v_1, v_2, v_3) for the legislative game. The member recognized is a (temporary) monopoly agenda setter, and chooses distributions x_2^1 and x_3^1 for the other two members subject to the restriction that (x_2^1, x_3^1) be in the feasible allocation set

$$A \equiv \{(X_2^1, x_3^1) \mid x_2^1 + x_3^1 \leq 1, \\ x_2^1 \geq 0, x_3^1 \geq 0\}.$$

The objective of member 1 is to maximize allocation, $x_1^1 = 1 - x_2^1 - x_3^1$, so indifference curves in the x_2^1-x_3^1 plane are linear and parallel to the upper boundary of the allocation set. Member 1 thus maximizes $1 - x_2^1 - x_3^1$ subject to $(x_2^1, x_3^1) \in A$ and subject to the requirement that his or her proposal obtains the vote of at least one other member.

A member j, $j = 2, 3$, will vote for member 1's bill if and only if $x_j^1 \geq \delta v_j$. A continuation set Z, defined as those allocations that will not command a majority, is thus

$$Z \equiv \{(x_2^1, x_3^1) \mid 0 \leq x_j^1 < \delta v_j, j = 2, 3\},$$

which is the interior of the square in Figure 3. Graphically, member 1 chooses an allocation (x_2^1, x_3^1) on the lowest indifference curve such that (x_2^1, x_3^1) is in the majority outcome set $M \equiv A - Z$. Since $v_j = \delta/3, j = 1, 2, 3$, two points—$(0, \delta/3)$ and $(\delta/3, 0)$—in the majority outcome set lie on the lowest indifference curve. The member recognized thus offers the continuation value to one other member and nothing to the other.

To identify the role of majority rule, suppose that the legislature operated under a unanimity rule so that member 1 must secure the votes of both of the other members. This would require that $x_2^1 \geq \delta v_2$ and $x_3^1 \geq \delta v_3$. The unanimity set U may thus be defined as

$$U \equiv \{(x_2^1, x_3^1) \mid x_j^1 \geq \delta v_j, j = 2, 3, \\ x_2^1 + x_3^1 \leq 1\} \cap A,$$

which is illustrated in Figure 3. Member 1 thus maximizes $1 - x_2^1 - x_3^1$ subject to $(x_2^1, x_3^1) \in U$. The solution is clearly $x_j^1 = \delta v_j$, $j = 2, 3$. Majority rule thus allows the member recognized to capture the share of the member excluded from the majority.

Since a majority can exclude a minority and distribute the benefits among themselves, the recognized member has an incentive to exclude as large a minority as possible. A minimal winning majority will thus be formed with the benefits distributed among the members of that majority. To determine how the benefits will be distributed, consider the case in which $\delta = 1$. Majority rule allows the one-third share of the excluded member to be divided between the member recognized and the other member of the majority, and agenda power allows the member recognized to capture that entire share. With discounting, that member also captures a premium of $(1 - \delta)/3$ due to the impatience of the other member.

For an n-member legislature the distribution of the benefits is δ/n for $(n - 1)/2$ members, zero for $(n - 1)/2$ other members, and $[1 - \delta(n - 1)/2n]$ for the member recognized. The latter is a decreasing function of n and is bounded above by two-thirds and bounded below by one-half. The difference between the benefits received by the proposer and the benefits δ/n received by the other members who vote for the proposal is an increasing function of n, so the distribution becomes more unequal as the size of the legislature increases. These properties are the same as those for the two-session game studied in the "Illustration." The stationary equilibrium characterized in proposition 3 is not the limit of all the equilibria characterized in proposition 1 of the finite session legislative game. It, however, is the limit of the finite session equilibrium in which at the beginning of each session the member recognized randomizes among the other members to determine whose vote will be attracted.

While proposition 3 is established in the special case of simple majority rule, the extension is straightforward to the case of k-member majority rule in which k members are required to adopt a proposal. For an n-member legislature governed by k-majority rule, random recognition, and a closed rule, the stationary equilibrium strategies are for the member recognized to offer δ/n to $k - 1$ members and to keep $1 - \delta(k - 1)/n$. Each member votes for any proposal offering at least δ/n. Note that as k goes to n, the benefits retained by the initial proposer go to $1 - \delta(n - 1)/n$ which, for $\delta = 1$, equals $1/n$. This represents the unanimity distribution under stationary strategies.[17] As k goes to one, the member recognized receives all the benefits. This may be thought of as corresponding to a property right granted to the member recognized.

An Application: Government Formation in Parliamentary Systems

One application of this framework is to the government formation problem in multiparty parliamentary systems when no party has a majority of seats. The benefits can then be interpreted as the ministries that are to be allocated among the parties in the majority. For simplicity of interpretation, assume that there are three parties and that the probability p_i of being asked to form a government is closely related to the proportion of seats held in the legislature after an election.[18] The bargaining over the allocation of ministries in a coalition government, then, corresponds closely to the model of a legislature operating under a closed rule: the party asked to form a government makes a proposal for allocating ministries (the benefits) among the parties, knowing that if its proposal does not receive a majority, another party (perhaps itself) will be asked (using the same recognition probabilities) to form a government.[19] The game thus has an infinite number of sessions, and the values v_i represent the expected proportion of ministries each party will have in the government. Attention will be restricted to stationary equilibria, and an argument analogous to that used to establish proposition 3 yields the stationary equilibria and provides predictions of which governments will form as well as how the ministries will be distributed. With asymmetric probabilities of recognition, however, asymmetric randomized strategies may result in equilibrium.

Let r_i denote that the probability that party i makes an offer of δv_{i+1} to party $i + 1$ (mod 3), where δv_{i+1} is the proportion of ministries allocated to party $i + 1$. Party 1, for example, makes a proposal $x^{1,2} = (1 - \delta v_2, \delta v_2, 0)$ with probability r_1 and a proposal $x^{1,3} = (1 - \delta v_3, 0, \delta v_3)$ with probability $1 - r_1$. In general, necessary conditions for a stationary equilibrium are that the values v_i and the randomization probabilities $r_i \in [0, 1]$ satisfy

$$v_1 = p_1[1 - r_1\delta v_2 - (1 - r_1)\delta v_3] + p_2(1 - r_2)\delta v_1 + p_3 r_3 \delta v_1 \qquad (1)$$

$$v_2 = p_2[1 - r_2\delta v_3 - (1 - r_2)\delta v_1] + p_3(1 - r_3)\delta v_2 + p_1 r_1 \delta v_2 \qquad (2)$$

$$v_3 = p_3[1 - r_3\delta v_1 - (1 - r_3)\delta v_2] + p_1(1 - r_1)\delta v_3 + p_2 r_2 \delta v_3. \qquad (3)$$

In equilibrium, a party i will adopt a strictly mixed strategy $r_i \in (0, 1)$ only if the continuation values of the other two parties are equal; and if two parties use strictly mixed strategies, all three continuation values will be equal. In the case in which two parties randomize, the values are $v_i = 1/3$, $i = 1, 2, 3$. Then, the rank of the matrix of coefficients of (r_1, r_2, r_3) in equations 1–3 is two, so there may be a continuum of equilibria. As an example, with $\delta = .8$, $p_1 = .45$, $p_2 = .35$, $p_3 = .2$, the equilibrium randomizations parameterized on r_3 are $r_1 = .19 + .44r_3$ and $r_2 = .96 + .57r_3$, for $r_3 \in [0, .0625]$.

To see why the continuation values can be equal even though the recognition probabilities are unequal, note that since the smallest party 3 is least likely to be recognized to form a government, its continuation value (out of equilibrium) would be expected to be smaller than the continuation values of the other two parties. The two largest parties would thus prefer to form a government with the smallest party. The smallest party would recognize this preference and, to join a government, would require a higher allocation of ministries than would be suggested by the likelihood it would be asked to form a government. If the smallest party were to demand more than one-third, however, at least one of the other parties would prefer to form a government with a party other than the smallest. In equilibrium the values thus must be equalized.

For the case in which $r_3 = 0$, party 1 with probability .19 offers to form a government with party 2 and with probability .81 offers to form a government with party 3. Party 2 with probability .96

offers to form a government with party 3 and with probability .04 offers to form a government with party 1. The probabilities ϱ_i that party i will be in the government are then $\varrho_1 = .46$, $\varrho_2 = .64$, $\varrho_3 = .90$. The smallest party is thus most likely to be in the government. In this example, however, the probability is .10 that the government will be formed by the two largest parties.

Amendments: A Simple Open Rule

With an open rule, proposals on the floor may be subject to amendment. This provides an opportunity for a member not allocated his or her continuation value in the proposal on the floor to make a substitute proposal. A substitute can always be chosen that will defeat the proposal on the floor, so the excluded member has an opportunity to assume the position of the member who made the prior proposal. The power of the member recognized first is thus diminished. That member, however, will take into account the possibility of an amendment in making an initial proposal. The issues to be investigated are thus the effect of the possibility of amendments on the size of the majority and on the distribution among the members of the majority and whether the possibility of an amendment may cause the legislature not to conclude its task in the first session. The characterization of the equilibrium for the case of an open rule also provides the basis for the analysis of the ex ante choice of an amendment rule, as considered later.

In the legislative process illustrated in Figure 2, each member has a $1/n$ probability of gaining initial recognition to make a proposal. Then each of the other members has a $1/(n - 1)$ probability of being recognized and may either make an amendment or move the previous question. If the previous question is moved, the proposal on the floor is voted on; and if it is approved, the benefits are distributed. If an amendment is offered, it must be voted against the proposal before either another amendment or moving the previous question is in order.[20] The winner of the vote then becomes the motion on the floor, and the next session begins. Each member other than the author of the proposal on the floor then has a probability of $1/(n - 1)$ of recognition for the purpose of offering an amendment or moving the previous question.[21] The process continues in this manner until the previous question is moved on a proposal and the proposal passes. In this setting, discounting is assumed to occur whenever an amendment is moved and voted on.

Under a simple open rule no member will ever be in a position to make a proposal or an amendment that cannot be pitted directly against another proposal. Thus, whoever proposes a bill or an amendment must take account of the fact that other members may subsequently be recognized. Another difference between an open rule and a closed rule is that a vote takes place only after a member has been recognized. The member initially recognized thus faces a trade-off. If the member wants to be certain that the proposal will be accepted, it must be sufficiently attractive that whoever is recognized next will move the previous question and then vote for the proposal. The equilibrium in this case would exhibit universalism. Making a proposal attractive to all members is expensive, however, so the member recognized may prefer to offer a proposal that is attractive to a subset including only m of the $n - 1$ other members. Then, if one of the m is recognized next and has been allocated at least as much as his or her continuation value corresponding to offering an amendment y, the member will move the previous question and, if $m + 1 \geq (n + 1)/2$, a majority will approve it. That continuation value will be denoted $\delta V_j^m(y)$, which is the discounted value of the game for j when j makes a successful amendment y and the legislature continues to the next session. If one of the $n - 1 - m$ other members is recognized, the member will offer an amendment chosen so as to defeat the motion on the floor, and the legislature will move to the next session. The member recognized thus can determine through the choice of m the likelihood that the proposal will be accepted. Intuition suggests that the lower the discount factor δ, the greater the incentive to offer a proposal that will be accepted with high probability, since continuing to the next session is then more costly. The higher the discount factor, the less costly continuation and the smaller the majority m to whom benefits are allocated. The majority m characterized in the following proposition is a (weakly) decreasing function of δ.

The equilibrium for a legislature with a simple open rule is characterized in the following proposition. To simplify the analysis, a member who is indifferent between two proposals will be assumed to vote for the one proposed last.[22]

PROPOSITION 4. *In an n-member, majority-rule legislature governed by a simple open rule and equal probabilities of recognition, a stationary equilibrium is a strategy configuration in which (1) the member recognized first makes a proposal* $\hat{y}$ *that allocates* $[(1 - \hat{y}^a)/m(\delta, n)]$ *to* $m(\delta, n)$ *other members, where* $(n - 1)/2 \leq m(\delta, n) \leq n - 1$, *and allocates* $\hat{y}^a$ *to him- or herself; (2) if one of those* $m(\delta, n)$ *members is recognized next, this member moves the previous question, the proposal is approved by the proposer of* $\hat{y}$ *plus the* $m(\delta, n)$ *members, and the legislature adjourns; and (3) if one of the* $n - 1 - m(\delta, n)$ *other members is recognized next, that member offers an amendment that allocates* $\hat{y}^a$ *to him- or herself and* $[(1 - \hat{y}^a)/m(\delta, n)]$ *to* $m(\delta, n)$ *other members excluding the member that made the previous proposal. Those members include all those offered zero in the proposal on the floor plus* $n - 1 - m(\delta, n)$ *others chosen randomly from those offered* $[(1 - \hat{y}^a)/m(\delta, n)]$ *in that proposal. This amendment defeats the prior motion and becomes the motion on the floor. Then repeat strategies 2 and 3. The number* $m(\delta, n)$, *defined in equation 4 is a weakly decreasing function of* δ *and a weakly increasing function of* n. *The function* $m(\delta, n)$ *is given by*

$$m(\delta, n) \in \arg_m \max V^m(\hat{y}), \tag{4}$$

where

$$V_1^m(\hat{y}) = \left(\frac{m}{n-1}\right) / \{1 + \delta\left(\frac{m^2}{n-1}\right)$$

$$- \delta^2\left(1 - \frac{m}{n-1}\right)\left[\left(\frac{1}{n-1}\right)\right.$$

$$\left. + \left(1 - \frac{m+1}{n-1}\right)\gamma(\delta, m\ n)\right]\};$$

$$\gamma(\delta, m, n) = \frac{\delta}{B}\left[\left(\frac{m}{n-1}\right)\right.$$

$$+ \delta\left(1 - \frac{m}{n-1}\right)\left(\frac{n-m-2}{m}\right)$$

$$\left.\left(\frac{1}{n-1}\right)\right];$$

and

$$B = 1 - \left(\frac{2m-n+2}{m}\right)\left(1 - \frac{m}{n-1}\right)$$

$$\delta - \left(\frac{n-m-2}{m}\right)\left(1 - \frac{m+1}{n-1}\right)$$

$$\left(1 - \frac{m}{n-1}\right)\delta^2.$$

The benefits $\hat{y}^a$ *retained by the author of the winning proposal are* $\hat{y}^a = 1 - m(\delta, n)\delta V_1^m(\hat{y}^1)$.

Table 1. Open Rule, Stationary Equilibria

Discount Factor (δ)	Size (n)	Majority ($\hat{m}[\delta,n]$)	Share of Proposer $\hat{y}^a$	Values of Games $V_1^m(\hat{y}_1^a)$	$V_k^m(\hat{y}_1^a)$	$V_j^m(\hat{y}_1^a)$	Probability First Proposal Approved
1	3	1	.60	.40	.40	.20	.5
1	5	2	.44	.28	.23	.12	.5
1	51	25	.06	.04	.03	.01	.5
1	101	50	.03	.02	.01	.01	.5
.8	3	1	.68	.40	.27	.16	.5
.8	5	2	.52	.30	.17	.09	.5
.8	51	25	.08	.05	.02	.01	.5
.8	101	50	.04	.02	.01	.00	.5
.6	3	2	.45	.45	.27	[a]	1.0
.6	5	2	.42	.33	.13	.07	.5
.6	51	25	.11	.06	.02	.01	.5
.6	101	50	.06	.03	.01	.00	.5
.4	3	2	.56	.56	.22	[a]	1.0
.4	5	3	.52	.40	.13	.04	.75
.4	51	25	.17	.08	.02	.00	.5
.4	101	50	.09	.05	.01	.00	.5
.2	3	2	.71	.71	.14	[a]	1.0
.2	5	4	.56	.56	.11	[a]	1.0
.2	51	25	.29	.14	.01	.00	.5
.2	101	50	.17	.08	.01	.00	.5

[a] No member receives zero in a proposal.

The proof is presented in the Appendix. The number $m(\delta, n)$ of members who receive benefits in the equilibrium proposal is a (weakly) decreasing function of δ, so the higher the discount factor, the smaller the majority. This results because the higher δ, the smaller the loss from continuing to the next session. Table 1 presents $m(\delta, n)$ for legislatures of varying sizes. As observed above, the legislature will complete its task in the first session with probability one only if the equilibrium proposal is universalistic. As indicated in Table 1, this will result only if δ is low. Similarly, the majority may be larger than the minimum, although as the size n of the legislature increases the closer $m(\delta, n)$ is to a minimal majority. If the model is interpreted as representing n blocks of legislators, each of which will share benefits equally among their members, a legislature with three blocks, for example, may yield universalistic outcomes if impatience is sufficiently great.

As indicated in Table 1, even when the majority is of minimal size under a simple open rule, the distribution among the members is more equal than under a closed rule. A member who votes for the winning proposal receives $[(1 - \hat{y}^a)/m(\delta, n]$, and the member who makes the winning proposal receives $1 - m(\delta, n)\{(1 - \hat{y}^a)/[m(\delta, n)]\} = \hat{y}^a$. For a legislature with 101 members and $\delta = 1$, $\hat{y}^a = .032$ and $[(1 - \hat{y}^a)/m(\delta, n)] = .019$. Under a closed rule, the member making the winning proposal receives .5, and the members voting for the proposal receive .01. An open rule thus greatly reduces the power of the member recognized compared to a closed rule even though in a large legislature without great impatience the benefits are distributed among a minimal majority under both rules.

Recognition is still valuable, however, since the value for the member recognized is at least as great as the value to any other members. To illustrate the value of recognition, consider the case of a three-member legislature. As indicated below, $m(\delta, 3) = 1$ if $\delta \in [\sqrt{3} - 1, 1]$ and $m(\delta, 3) = 2$ if $\delta \in [0, \sqrt{3} - 1]$. For $\delta \in [\sqrt{3} - 1, 1]$, let $V_2^1(\hat{y}^1)$ and $V_3^1(\hat{y}^1)$ denote, respectively, the values of the game to the member offered $1 - \hat{y}^a$ and to the member offered zero in the proposal $\hat{y}^1 = \hat{y}$ of member 1, where the superscript is $m(\delta, 3)$. The values are $V_1^1(\hat{y}^1) = 2/(4 + 2\delta - \delta^2)$, $V_2^1(\hat{y}^1) = 2\delta/[(2 - \delta)(4 + 2\delta - \delta^2)]$, and $V_3^1(\hat{y}^1) = \delta/(4 + 2\delta - \delta^2)$. It is straightforward to show that $V_1^1(\hat{y}^1) + V_2^1(\hat{y}^1) + V_3^1(\hat{y}^1) < 1$ for $\delta \in (\sqrt{3} - 1, 1)$. For $\delta = 1$, the continuation values are $V_1^1(\hat{y}^1) = 2/5$, $V_2^1(\hat{y}^1) = 2/5$, and $V_3^1(\hat{y}^1) = 1/5$. For the case of a closed rule, the values of the game conditional on being recognized are, for $\delta = 1$, two-thirds, one-third, and zero respectively. Consequently, compared to the equilibrium with a closed rule, the value of recognition is lower and the values of the game are closer together with an open rule. The possibility of amendments thus substantially reduces the value of recognition.

As δ increases, $V_1^1(\hat{y}^1)$ decreases, so (as under a closed rule) whoever is recognized first benefits from greater impatience. For $\delta < \sqrt{3} - 1$ the member recognized first prefers to make a universalistic $[m(\delta, 3) = 2]$ proposal, and the value for member 1 is $V_1^2(\hat{y}^1) = \hat{y}^a = 1/(1 + 2\delta)$. The values for the other two members equal $(1 - \hat{y}^a)/2 = \delta/(1 + 2\delta)$. The sum of the values equals one, since the previous question is moved by whoever is recognized next. The value $V_1^2(\hat{y}^1)$ of the member recognized first is a decreasing function of δ and is less than the corresponding value under a closed rule for $\delta > 0$.

In summary, the principal features of the stationary equilibrium for a simple open rule are the following: First, if impatience is sufficiently low, the benefits are distributed among a minimal majority; but if impatience is high, the benefits may be shared among a supermajority. Second, the initial proposal is not necessarily accepted in the first session when impatience is low. Instead, with some probability an amendment is offered and the legislature continues to the next session. When impatience is low, the probability thus is positive that the legislature will not reach an agreement in any finite number of sessions; thus, the sum of the continuation values is less than one. Third, as indicated in Table 1, the value to the member initially recognized is greater, the greater the impatience of the members. Fourth, the benefits are distributed more evenly among the winning majority under an open rule than under a closed rule. Fifth, even though amendments may be offered in equilibrium, the initial proposer is still advantaged. Recognition is valuable; but, as indicated in Table 1 and proposition 3, recognition is not as valuable as under a closed rule.

The Choice of an Amendment Rule

Legislatures often are delegated the duty of choosing their own method of organization; and in the model considered here, that choice corresponds to the selection of an amendment rule. In this model, that selection is made at the ex ante stage before any member has gained recognition to make a proposal; so all members are in a symmetric position and thus have identical preferences over amendment rules.[23] The members, however, weakly prefer a closed rule because it assures that delay will not result. Whenever delay is costly (but not so costly that a universalistic proposal is made under an open rule), all members strictly prefer a closed rule to an open rule; that is, for all $\delta < 1$ such that $m(\delta, n) < n - 1$, the sum of the values of the game is strictly less under an open rule than under a closed rule. Because each member has the same chance of being recognized, each member prefers a closed rule.[24]

Of course, the distribution problem studied here is not representative of the bulk of the proposals that legislatures consider, so the extent to which these results generalize is not clear. The basic implication of this analysis, however, conforms with political intuition. If delay in the passage of legislation is costly but is not so costly that immediate passage is a necessity, strategic considerations can come into play. The incentive to use the opportunity to make a proposal to benefit one's own district is strong, and under an open rule the pursuit of that opportunity can result in costly delay. In the context of the distribution problem and legislative process considered here, that cost can be avoided by adopting a closed rule.

At least in the Congress, this prediction is not supported by the evidence; so it is useful to explore in more detail the structure of the model considered here and the additional considerations needed to provide a complete theory of the choice of an amendment rule. First, the legislature modeled here does not have committees with jurisdictions, so it is natural to represent each member as having the same probability of recognition. If a committee had the right to make the first proposal under a closed rule, it would, if modeled as a unitary actor, have an ex ante value of the game equal to $1 - [(n - 1)/2](\delta/n)$, and the rest of the members would have an expected value equal to only $(\delta/2n)$. In such a case, the legislature would prefer an open rule to a closed rule as a means of diminishing the power of the committee. We investigate this subject in more detail in Baron and Ferejohn 1989.

A legislature might adopt an open rule for a variety of other reasons. An open rule results in more equal ex post distributions than does a closed rule, so it might be chosen to further equity considerations. Similarly, risk aversion on the part of members of the legislature could result in a preference for the less unequal ex post distribution that results with an open rule.[25] Furthermore, an open rule seems to comport better with democratic theory in the sense that it allows greater opportunity to members than does a closed rule, which immediately closes the amendment process when a proposal is made. Whereas these considerations may explain the relatively infrequent use of a closed rule, the theory presented here indicates that the cost of an open rule is the possibility of delay.

In practice, the House of Representatives employs a variety of amendment rules to govern floor consideration of legislative proposals. Krehbiel (1989) and Bach and Smith (1988) found that a closed rule is not used frequently in the House, although much legislation is considered under modified rules. These rules do restrict the amendments that can be made on the floor and thus serve to reduce the likelihood of delay. Similarly, the Senate operates without amendment rules, but it has increasingly resorted to unanimous consent agreements to limit time or amendment activity.

Conclusions

We provide a theory of a majority rule legislature in a context in which the standard institution-free model of social choice theory yields no equilibrium. The structural aspect of the legislature that yields equilibria is the sequential process by which proposals are made and voted on. The member recognized to make a proposal is shown to have agenda power that results from the chance that a member may be excluded by a majority in a future session. Impatience adds to that agenda power. With a closed rule, the equilibrium outcomes are majoritarian, the first proposal is passed, and benefits are distributed to a minimal majority. Since each member acts noncooperatively, the majority is not a coalition in the sense the term is used in cooperative game theory. Compared to a closed rule, the opportunity to make an amendment under an open rule dramatically reduces the agenda power of the member recognized first and results in an outcome that more evenly distributes the benefits among the winning majority. Unless impatience is great, however, the distribution is majoritarian, and the proposal made by the first member recognized is not necessarily accepted. If, however, the number of members is small and there is substantial impatience (as there would be if members faced a significant electoral risk), the equilibrium is "universalistic" in the sense that every member receives benefits. Thus, if a legislature has a few internally homogeneous blocks that can act in a concerted manner, universalistic outcomes might be expected if impatience is substantial. The member recognized to make a proposal nevertheless retains some agenda power and receives more benefits than any other member.

The amendment rule used by a legislature is endogenous, and the theory presented here predicts that unless there is no impatience, the legislature prefers a closed rule to a simple open rule. This results in part because the internal structure of the legislature is not modeled; and if that structure (e.g., a committee system) gave agenda power to a particular individual, the legislature might be expected to adopt an open rule that to some extent lessened that power. Predictions about the implications of the internal organization of legislatures from this perspective await further research.

These predictions of the noncooperative bargaining theory of legislatures contrast with those that arise in cooperative models that focus on coalition formation. Cooperative models of politics abstract from the process by which alternatives arise and assume that coalitions will freely form to defeat alternatives when a majority of members prefers another available alternative. Cooperative theories generate weak predictions about legislative outcomes because they ignore the sequential aspects of legislative consideration of alternatives and ignore the implications of structure for undertaking coordinated activity (or coalition formation) in a legislative setting. One prediction from this approach is Riker's minimal winning coalition theorem. The noncooperative theory presented here demonstrates that Riker's conclusion holds under a closed rule and also under a simple open rule if impatience is sufficiently small or the legislature is large. Using this bargaining model, Baron (1989) considers the possibility of coordinated proposal making (but not voting) among members and finds that benefits are allocated to more than a minimal number of districts but not to all.

By taking seriously the sequential structure of legislative action, definite predictions about the outcomes of legislative processes are obtained. In addition to the paradigmatic case of the U.S. Congress, for which our theory provides a strong prediction as to how a single distributional issue will be resolved, the theory has implications for the problem of cabinet formation in multiparty democracies. It is also clear that once the preferences of the members are specified, the bargaining theory approach yields predictions about the relations between the president and Congress in the appointments process (in which the president monopolizes the proposal power) and in the legislative process (in which Congress monopolizes the proposal power).[26] Indeed, in any setting in which the proposal-making process can be clearly specified, the bargaining approach will generate testable predictions.

Finally, it seems appropriate to speculate about repetition. Our model is one in which a fixed quantity of benefits is to be distributed once and for all. In actual legislatures, many issues are decided; and the members may wish to make use of voting and proposal strategies that link their behavior on certain issues to those on others. Our model is useful in considering this richer setting in several senses. First, it represents a useful way to think about the resolution of the "final" issue, if there is one. In this sense, there is a finite set of issues and backwards induction can be used to characterize equilibria for the multi-issue legislature. Second, when there are multiple issues to be resolved, a construction of the sort given in proposition 2 is available; so a great variety of legislative outcomes may be supported by nonstationary strategies.[27] Third, if multiple issues are considered simultaneously, different members (e.g., committees) would be recognized to make proposals in their policy jurisdictions. Then, if preferences were as represented here, more universalistic outcomes might be expected. Since universalistic outcomes also result under an open rule if impatience is sufficiently great and the number of members or blocks is small, stationary equilibria may still have some importance as a focal point in a repeated setting.

Appendix

Proof of Proposition 2.[28] The proof requires that the punishment in strategy 3 make any deviation $y \neq x$ yield a continuation value of zero for the deviator j. This requires specification of $z(y)$ and the majority M. Given $y \neq x$, choose some majority M of members that does not contain j, and let $Y_M(y) \equiv \Sigma_{i \in M} y_i$ denote the total distribution to that majority. Let $M^*(y)$ denote a majority with the smallest cost or

$$M^*(y) \in \arg \min_{M \in M_j} Y_M(y),$$

where M_j is the set of all majorities not containing j. The number of members of $M^*(y)$ is $(n + 1)/2$ and let m^* be the number of members of $M^*(y)$ such that $y_i = 0$, $i \in M^*(y)$. Then define the distribution $z(y)$, for $i \in M^*(y)$, by

$$z_i(y) = \begin{cases} \dfrac{y_i}{Y_{M^*(y)}(y)} - \eta\epsilon & \text{if } y_i > 0 \\ \epsilon & \text{if } y_i = 0, \end{cases}$$

where $\epsilon > 0$ and small, $\eta = m^*/[(n + 1)/2 - m^*]$ if $m^* < (n + 1)/2$, and $\eta = 0$ if $m^* = (n + 1)/2$. For $i \notin M^*(y)$, let $z_i(y) = 0$. By construction, the proposal $z(y)$ is such that $z_i(y) > y_i$, $\forall i \in M^*(y)$. All members of the majority $M^*(y)$, knowing that each subsequent proposal will be $z(y)$ and that it will be approved, will thus reject y_i if δ is such that

$$\delta z_i(y) > y_i \, \forall i \in M^*(y). \qquad \textbf{(A-1)}$$

The majority knows this because it anticipates that any deviation from the proposal $z(y)$ will be punished for the same reason that the proposer of y is being punished.

Next, the values of δ and n such that the inequality in relationship A-1 is satisfied must be determined. For $y_i = 0$, relationship A-1 is satisfied for all $\delta > 0$; and for $y_i > 0$, the inequality implies that

$$\delta > \frac{y_i}{z_i(y)} = \frac{y_i}{[y_i/Y_{M^*(y)}(y)] - \eta\epsilon} = \frac{Y_{M^*(y)}(y)}{1 - \eta\epsilon Y_{M^*(y)}(y)/y_i} \quad \forall i \in M^*(y).$$

Since x is arbitrary, the least upper bound on $Y_{M^*(y)}(y)$ must be determined. It is straightforward to verify that if ϵ is sufficiently small, the least upper bound Y^* on $Y_{M^*(y)}(y)$ is

$$Y^* \equiv \max_y Y_{M^*(y)}(y) = \begin{cases} (n + 1)/[2(n - 1)] & \text{if } n \text{ is odd,} \\ (n + 2)/[2(n - 1)] & \text{if } n \text{ is even.} \end{cases}$$

The upper bound Y^* is determined by the deviation y that is most costly to defeat, that is, $y = (1/(n-1), \ldots, 1/(n-1), 0, 1/(n-1), \ldots, 1/(n-1))$, where the zero is in the position for the deviator j. For n odd, a majority thus requires $1/(n-1)$ for $(n+1)/2$ members. Thus, an arbitrary x can be supported in equilibrium if $\delta > Y^* = (n+1)/[2(n-1)]$. For n odd, such a δ exists if $n \geq 5$; and for n even, such a δ exists if $n \geq 6$. Any deviator j will thus find his or her proposal y defeated by anticipating that $z(y)$ will be passed, in which case the deviator receives zero.

The remaining case to consider is that in which the deviator is recognized in consecutive sessions. Strategy 3 thus calls for the deviator to punish him- or herself, but suppose that j deviates from that as well. Then if in the next session a member $k \neq j$ is recognized, k will punish j. The only event in which j can avoid punishment is if he or she is recognized in every session; but no majority will vote for the deviation, so $\lim_{\tau \to \infty} \delta^\tau y_j = 0$ for all $\delta < 1$.

Any deviation thus yields zero to the deviator. Thus, no member has an incentive to deviate, and x is supported as a subgame-perfect equilibrium. QED

Proof of Proposition 3. Sufficiency follows from direct verification that the configuration of offers is an equilibrium in every subgame. To establish necessity, first note that all subgames including the entire game are structurally identical. Let v_i denote the stationary continuation value for member i, $i = 1, \ldots, n$, for each subgame and let the proposal made by member i when recognized be denoted by $x^i = (x_1^i, \ldots, x_n^i)$. Any member recognized will prefer to make a proposal that will be accepted, since making a proposal that will be rejected is equivalent to making no proposal. Subgame perfection implies that member 1 need be no more generous than to offer the discounted continuation value to a minimum number of members and zero to the others. Consequently, if a member, j, is recognized, that member will offer x^j such that $x_i^j \geq \delta v_i$ to $(n-1)/2$ other members needed to constitute a majority. Any member receiving an offer $x_i^j \geq \delta v_i$ will vote for it. Consider then the case in which member i chooses at random a bill that attracts the vote of $(n-1)/2$ members; that is, the probability that a member will receive a positive share of the benefits is one-half. The continuation values v_i are, using symmetry,

$$v_i = \frac{1}{n}\left[1 - \frac{n-1}{2}\,\delta v_i\right] + \left(\frac{n-1}{n}\right)(1/2)\delta v_i, \quad i = 1, \ldots, n,$$

where the first term corresponds to the event that i is recognized and the second term corresponds to the event that one of the other members is recognized. This simplifies to $v_i = 1/n$, $i = 1, \ldots, n$. Then $x_i^j = 1 - \delta(n-1)/2n$, $i = 1, \ldots, n$; and $x_i^j = \delta/n$, $i = 1, \ldots, n$, $i \neq j$. QED

Proof of Proposition 4. A constructive proof will be presented. Because of the symmetry of the legislature, the member (denoted member j) recognized first will make a proposal y^j that offers $(1 - y^a)/m$ to m other members selected at random—where $(n-1)/2 \leq m \leq n-1$—and offers y^a to him- or herself. It can be shown that recognition is valuable in this game, which is to say that $y^a \geq (1 - y^a)/m$ so that member j receives at least as much as any other member. Let $V_j^m(y^j)$ denote the value of the game to member j when proposal y^j is on the floor and let $\hat{y}^j$ denote the proposal that maximizes $V_j^m(y^j)$, so $\hat{y}^a$ denotes the allocation to the proposer. A member i to whom $(1 - \hat{y}^a)/m$ is offered will move the previous question rather than make an amendment if and only if

$$\frac{1 - \hat{y}^a}{m} \geq \delta V_i^{m^*}(y^i), \tag{A-2}$$

where $V_i^{m^*}(y^i)$ is the continuation value to i if i proposes an amendment y^i that defeats $\hat{y}^j$. Note that if y^i defeats $\hat{y}^j$ and y^i becomes the proposal on the floor, then $V_i^{m^*}(y^i) = V_i^m(y^i)$. Then if $\hat{y}^j$ maximizes $V_j^m(y)$, then $\hat{y}^i$, defined by $\hat{y}^i_i = \hat{y}^a$ and $(1 - \hat{y}^a)/m$ offered to m other members selected at random, maximizes $V_i^{m^*}(y^i)$. Clearly the m members to be offered $(1 - \hat{y}^a)/m$ can be chosen such that $\hat{y}^i$ defeats $\hat{y}^j$.[29] Since $\hat{y}^a \geq (1 - \hat{y}^a)/m$, member j will be offered zero unless $m = n - 1$, in which case every member receives a positive amount of the benefits. Consequently, any member i who offers an amendment rather than moving the previous question will have a continuation value $V_i^{m^*}(\hat{y}^i) = V_i^m(\hat{y}^j)$. Consequently, relationship A-2 is

$$\frac{1 - \hat{y}^a}{m} \geq \delta V_j^m(\hat{y}^j). \qquad \text{(A-3)}$$

In the following, the member recognized first is denoted 1, a member who receives $(1 - \hat{y}^a)/m$ in 1's proposal $\hat{y}^1$ is denoted k, and j and l denote members who receive zero in $\hat{y}^1$. The value $V_1^m(\hat{y}^1)$ is given by

$$V_1^m(\hat{y}^1) = \frac{m}{n-1}\hat{y}^a + \left(1 - \frac{m}{n-1}\right)\delta V_1^m(\hat{y}^j),$$

where $m/(n - 1)$ is the probability that one of those offered $(1 - \hat{y}^a)/m$ is recognized and moves the previous question and $[1 - m/(n - 1)]$ is the probability that one, denoted j, of the members offered zero by member 1 is recognized. Member j will propose $\hat{y}^j$, and since $\hat{y}^a \geq (1 - \hat{y}^a)/m$, the component $\hat{y}^j_1 = 0$ for all j. The above argument indicates that $\hat{y}^j$ is a permutation of $\hat{y}^1$, so $V_1^m(\hat{y}^j) = V_j^m(\hat{y}^1)$ and thus

$$V_1^m(\hat{y}^1) = \frac{m}{n-1}\hat{y}^a + \left(1 - \frac{m}{n-1}\right)\delta V_j^m(\hat{y}^1). \qquad \text{(A-4)}$$

The value $V_j^m(\hat{y}^1)$ is given by

$$V_j^m(\hat{y}^1) = \frac{m}{n-1} \cdot 0 + \frac{1}{n-1}\delta V_j^m(\hat{y}^j) + \left(1 - \frac{m+1}{n-1}\right)\delta V_j^m(\hat{y}^l),$$

where $m/(n - 1)$ is the probability that a member, k, offered $(1 - \hat{y}^a)/m$ in 1's proposal will move the previous question, in which case $\hat{y}^1$ will be adopted; $1/(n - 1)$ is the probability that j is recognized and will successfully offer $\hat{y}^j$ yielding a value $V_j^m(\hat{y}^j)$; and $1 - (m + 1)/(n - 1)$ is the probability that another member l offered zero in member 1's proposal is recognized and offers an amendment $\hat{y}^l$. If member l who was offered zero in $\hat{y}^1$ is recognized, he or she will make a similar proposal that defeats $\hat{y}^1$, and that proposal $\hat{y}^l$ will allocate $(1 - \hat{y}^a)/m$ to j; so the value $V_j^m(\hat{y}^l)$ to j equals the value $V_k^m(\hat{y}^1)$ to k, given $\hat{y}^1$. Thus,

$$V_j^m(\hat{y}^1) = \frac{1}{n-1}\delta V_1^m(\hat{y}^1) + \left(1 - \frac{m+1}{n-1}\right)\delta V_k^m(\hat{y}^1). \qquad \text{(A-5)}$$

We can write the expression for $V_k^m(\hat{y}^1)$ as follows:

$$V_k^m(\hat{y}^1) = \frac{m}{n-1} \cdot \frac{1 - \hat{y}^a}{m} + \left(1 - \frac{m}{n-1}\right)\delta V_k^m(\hat{y}^l). \qquad \text{(A-6)}$$

Using relationship A-3, the expression for $V_k^m(\hat{y}^l)$ can be seen to take the following form:

$$V_k^m(\hat{y}^l) = \frac{2m - n + 2}{m} \cdot$$

$$[(\frac{m}{n-1})\delta V_1^m(\hat{y}^1)$$

$$+ (1 - \frac{m}{n-1})\delta V_k^m(\hat{y}^l)]$$

$$+ \frac{n-m-2}{m} [\frac{\mathrm{m}}{n-1} \bullet 0$$

$$+ \frac{1}{n-1} \delta V_1^m(\hat{y}^1)$$

$$+ (1 - \frac{m+1}{n-1})\delta V_k^m(\hat{y}^1)], \qquad \text{(A-7)}$$

where, with probability $(2m - n + 2)/m$, member k will receive $(1 - \hat{y}^a)/m = \delta V_1^m(\hat{y}_1)$ in $\hat{y}^l$, which in turn will be accepted with probability $m/(n - 1)$ or rejected with probability $1 - m/(n - 1)$. In the latter case k is in the same position as before, when $\hat{y}^l$ was proposed, so k's value is $\delta V_k^m(\hat{y}^l)$. With probability $(n - m - 2)/m$, k will receive zero in $\hat{y}^l$, and then one of three possibilities will occur: with probability $m/(n - 1)$, $\hat{y}^l$ will be accepted, and k will receive 0; with probability $1/(n - 1)$, k will be recognized to make a proposal, in which case he or she can be assured of $\delta V_1^m(\hat{y}^1)$; and with probability $1 - (m + 1)/(n - 1)$, someone else who received zero in $\hat{y}^l$ will be recognized and will make a proposal in which k will receive $(1 - \hat{y}^a)/m$, so that k will be in the same position as it was following the proposal of $\hat{y}^1$, and k's value at that point will be $\delta V_k^m(\hat{y}^1)$.

Substituting equation A-6 into A-1 and rearranging terms allows us to solve for $V_k^m(\hat{y}^1)$ in terms of $V_1^m(\hat{y}^1)$:

$$V_k^m(\hat{y}^1) = \gamma(\delta, m, n) V_1^m(\hat{y}^1), \qquad \text{(A-8)}$$

where

$$\gamma(\delta, m, n) = \frac{\delta}{B} [(\frac{m}{n-1})$$

$$+ \delta(1 - \frac{m}{n-1}) (\frac{n-m-2}{m})$$

$$(\frac{1}{n-1})]$$

and

$$B = 1 - (\frac{2m-n+2}{m}) (1 - \frac{m}{n-1})\delta$$

$$- (\frac{n-m-2}{m}) (1 - \frac{m+1}{n-1})$$

$$(1 - \frac{m}{n-1})\delta^2.$$

Then, substituting equations A-5 and A-8 and relationship A-3 into A-4 yields a closed form expression for $V_1^m(\hat{y}^1)$ or

$$V_1^m(\hat{y}) = (\frac{m}{n-1}) / \{1 + \delta(\frac{m^2}{n-1})$$

$$- \delta^2(1 - \frac{m}{n-1}) [(\frac{1}{n-1})$$

$$+ (1 - \frac{m+1}{n-1})\gamma(\delta, m, n)]\}. \qquad \text{(A-9)}$$

For $m = n - 1$, this simplifies to $V_1^m(\hat{y}^1) = 1/[1 + \delta(n - 1)]$. Member 1 will choose $m = m(\delta, n)$ to maximize $V_1^m(\hat{y}^1)$, so $m(\delta, n) \in \mathrm{argmax}_m\, V_1^m(\hat{y}^1)$. Then, $\hat{y}^a$ is determined from relationship A-2 as $\hat{y}^a = 1 - m(\delta, n)\delta V_1^m(\hat{y}^1)$. The values $V_k^m(\hat{y}^1)$ and $V_j^m(\hat{y}^1)$ are obtained by substituting equation A-9 into A-8 and equations A-9 and A-8 into A-5, respectively. QED

Notes

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1. See Sutton 1986 for a survey of these results.
2. If a unanimity rule is employed and if members are recognized sequentially, the same result is obtained by Dutta and Gevers (1981) in a finite session legislative model and by Herrero (1985) in a stationary equilibrium for an infinite number of sessions.

3. Other recognition rules are discussed briefly later.

4. Germaneness thus may be thought of as requiring proposals and amendments to be in the set X.

5. See Oleszek 1984.

6. That is, second-degree amendments are not allowed.

7. In the terminology of Ferejohn, Fiorina, and McKelvey (1987) such amendment rules exhibit a "depth" limitation. They are properly termed open rules because there is no limit to the number of amendments that may be offered before any proposal is brought to a vote.

8. See Moulin 1979.

9. If a member votes for a bill only if the member strictly prefers it, the distributions necessary to achieve a majority lie in an open set. Then an equilibrium does not exist, since for any distribution that achieves a majority, there is another distribution that also achieves a majority and is better for the proposer. To avoid these complications and the use of an equilibrium concept such as an ϵ-equilibrium, a member who is indifferent between a bill and continuing to the next session is assumed to vote for the bill.

10. The power of the proposer in this setting is investigated in more detail in Baron and Ferejohn 1989.

11. This observation is due to Douglas Rivers.

12. This observation has been made by Herrero (1985) who considered n-person bargaining under a unanimity rule and sequential recognition.

13. While we take the reelection probability to be exogenous for the present argument, the same caveat would apply to a more complete treatment of reelection.

14. Stationary equilibria have been studied by Herrero in the context of n-person bargaining under a unanimity rule. Binmore (1985) shows that if the strategies are required to satisfy a continuity condition on the histories, there is a unique equilibrium and the equilibrium strategies are stationary.

15. A stationary equilibrium provides a complete specification of the strategies of members on the equilibrium path. If a move off-the-equilibrium path is made, members are assumed to believe that a "mistake" has been made and that mistakes will not be made in subsequent play. Members will thus base their responses on the equilibrium path strategies.

16. Instead of randomizing among the members, a strategy configuration such that each member receives an offer of δ/n from exactly $(n - 1)/2$ other members (e.g., the $(n - 1)/2$ members on the left) is also a stationary equilibrium.

17. Dutta and Gevers (1981) also obtain this result.

18. One feature of government formation in parliamentary systems is that failure of a vote of confidence can dissolve the government. The assumption here is that an acceptable allocation of ministries is sufficient to maintain the government.

19. The analysis can be generalized to the case in which the probabilities of recognition are different at each recognition.

20. With the interpretation of the discount factor representing impatience rather than a reelection probability, the model can be understood as allowing a number a of amendments per session; that is, the discount factor can be expressed as $\delta(a)$, where $\delta(a) = \exp^{-i/a}$ and i is the time preference or interest rate. Then, $a = 1$ represents one amendment and vote in each session, which corresponds to the case of a closed rule or $\delta = \delta(1)$. If an open rule allows a number $a > 1$ amendments and votes per session, then $\delta(a) > \delta$.

21. Thus, there is never consecutive recognition.

22. Otherwise, the set of amendments that defeat the proposal on the floor is open, and the equilibrium concept has to be weakened to that of an ϵ-equilibrium.

23. That is, under a closed rule and under an open rule the ex ante values of the game are equal for each member.

24. Gilligan and Krehbiel (1987, 1989) provide another explanation for the use of a restrictive rule. They demonstrate that the parent chamber may grant a closed rule to generate incentives for a committee to develop information and expertise that the parent body can use to its benefit. The benefits from the information in their theory can offset the strategic advantage the closed rule gives to the committee.

25. See Harrington 1987 for a study of the effect of risk aversion in a bargaining context.

26. See Baron and Ferejohn 1989.

27. See Epple and Riordan 1987.

28. The construction in this proof is related to that in Herrero 1985.

29. To see this, note that member j who has $y^i_j = 0$ will vote for y^i if $y^i_j \geq 0$ and member k who has $y^i_k = (1 - y^a)/m$ will vote for y^i if $y^i_k \geq (1 - y^a)/m$.

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RULING: GUARDIANS AND PHILOSOPHER-KINGS

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According to some accounts, Plato views ruling as a philosophical activity; according to others, he understands it to be a craft. Given the apparent discrepancy between philosophical and technical endeavor, it seems that the two interpretations cannot both be correct. The issue is explored in light of the relationship between guardians and philosopher-kings as it manifests itself in the basic structure of the Republic *and also with respect to questions of education, character, and deceit.*

While modernity seems to have accepted the formula that politics is a matter of "ruling and being ruled in turn," it is Plato, not Aristotle, who is most often consulted for a theory of ruling itself—this despite the fact that Plato's account of ruling is particularly elusive. Consider, for example, the view that for Plato ruling is a craft. Such an interpretation is important for a certain tradition of contemporary political thought best exemplified by the work of Hannah Arendt. According to Arendt, Plato's concept of ruling is very likely his most substantial—and also most disturbing—contribution to the political life of the West; for in the *Republic* we encounter for the first time the idea that ruling is a matter of "making" rather than "doing" (1959, 197–206; see also Wolin 1960, 34–63). Political society and its laws do not emerge inexplicably from the ever-changing tissue of social and cultural life; nor do they arise out of great deeds and from the stories that those deeds somehow inspire. Rather, they are simply fabricated in terms of well-established principles and procedures by the artisans of the state. Ruling is essentially a technical matter of constructing or fashioning laws and institutions, a craftlike endeavor.

According to such an account, the traits and activities of the Platonic ruler will be very different from those of, say, the heroes of Homeric epic, the great founders and lawgivers, or even the protagonists of Thucydidean Greece. Whereas Pericles, for example, managed to establish or maintain political order by virtue of his own audacity, courage, and eloquence, the Platonic ruler would instead simply construct such an order out of available raw materials, utilizing a preexisting and rationally accessible blueprint and relying on certain well-founded and well-defined techniques or routines. A good carpenter is trained in using tools, judging the properties of wood, and reading and following plans. The carpenter who is also suited by temperament to a life of carpentry will be well able to craft objects that perform admirably the functions for which they are created. In a precisely analogous way, the guardians of the *kallipolis* will craft and execute the policies and laws of the city. Arendt writes of "the Platonic wish to substitute making for acting in order to bestow upon the realm of human affairs the solidity inherent in work and fabrication" (1959, 202). This is a seductive wish that has long influenced the Western conception of ruling and that, in Arendt's view, has turned our attention away from the real nature of political action.

A not-dissimilar interpretation was argued by M. B. Foster in his quite brilliant and underappreciated book on Plato and Hegel. Foster writes, "Both the positive achievement of the Platonic conception and its limitations appear most clearly in his doctrine that ruling is an art, or Technê; for it is characteristic of a 'technical' activity in the Greek sense of the word both that it should issue in the realization of a design which is itself unaffected by the process of realization, and that it should impose a form upon a material which is other than the craftman's self" (1965, 18).

Among other things, this implies that Plato's ruler must act solely in terms of the model before him, and must "possess antecedently to his work a clear scientific insight into the form which his work is to realize" (Foster 1965, 19). Foster's conclusions from this are, incidentally, quite controversial. He claims that the notion of ruling as *technē* is ultimately incoherent insofar as political society, unlike an object crafted, must include the ruler or "craftsman" as a part of itself. As a result the very concept of political society demands that we ascribe to it, to its internal nature, an element of free subjectivity, of sovereignty. According to Foster, this is the great insight of modern politics, an insight formulated most clearly by Hegel.

Be that as it may, it is clear that Foster, Arendt, and, indeed a host of other commentators think of the Platonic ruler as a craftsman, and tend to believe that there is substantial textual support for such a view (see, e.g., Annas 1981, 261; Bambrough 1956; Bambrough 1962, 97–113; Gadamer 1986, 281–89; Grube 1958, 265; and Strauss 1964, 71–79). But this interpretation also seems not to comport very well with the more standard notion that Platonic ruling is essentially a matter of philosophy; for philosophy, at least as Plato understands it, can hardly be considered a craft. It seems certain that Plato himself would want to distinguish the activity of philosophizing sharply from that of fabrication, hence the philosopher as philosopher from the craftsman as craftsman. And it is in light of this distinction that some have argued, again quite plausibly, that Plato in the *Republic* explicitly and pointedly rejects the so-called craft analogy (Cross and Woozley 1964, 12–16; Gould 1955, 31; Irwin 1977, 183–85, 201–3.

There is a sense, of course, in which both craftsman and philosopher are acquainted with the forms or essences of things. The former, however, is familiar with them only as models to guide activity: a craftsman's work is, above all, to create empirical instantiations of the models. The focus of a craftsman's attention, therefore, is largely on the world of sensuous appearance. The craftsman is immersed in this world, in the raw material, and seeks to shape it according to a particular intuited pattern. The philosopher's attention, on the other hand, turns away from the empirical world. The philosopher spends life, rather, in contemplation of the forms themselves and as a result is able to subject personal knowledge of the forms to a kind of perpetual critical scrutiny, so that beliefs about them can move ever closer to the truth. The philosopher is able to deepen understanding of them as the craftsman, concerned only with sensuous things, cannot. A craftsman's success is measured solely in terms of particular objects contributed to what is, in Plato's judgment, the insubstantial, shadow-world of appearances. A philosopher, to the contrary, makes nothing and is uninterested in shadows, thinking only about reality.[1]

The entire issue is complicated, of course, by serious questions of language and translation. Most English-language translators render *technē* as "craft" (or sometimes, alternatively, as "art"). But they also generally use *craftsman* as a translation of *dēmiourgos.* Obviously, the etymological connection found in the English words is utterly lacking in the original Greek. Moreover, if we are to believe Liddell, it seems that the connotations of *dēmiourgos* are rather broader than those of its "etymologically preferred" alternative, *technitēs.* Whereas the latter directly implies one skilled in following the rules of a particular *technē,* the former connotes a more generalized sense of skillfulness involving any kind of activity, craft or noncraft. Thus, when Plato specifically refers to the philosopher-king as a *dēmiourgon* (*Republic,* 500d), it is by no means clear that this is best rendered as "craftsman" in the sense of one skilled in *technē.* Indeed, the context of the argument, in which the philosopher-king is repeatedly likened to a painter (*zōgraphos*), suggests a substantial difference between the skill of a philosopher-king and that of someone engaged in a *technē*—especially if we warrant the deep distinction that Plato later makes (597d–598d) between painters and other *dēmiourgoi.*

To be sure, one might imagine a philosopher who chose to make things in leisure time, time away from philosophy. But it remains clear that for Plato the philosopher must be a different *kind* of person from the craftsman; for insofar as the philosopher's work and primary function is to think rather than make, the philosopher presumably must be quite distinctive in terms of training, temperament, habits, interests, and the like. It seems also quite clear that a philosopher who occasionally made things would not necessarily do so any better than the craftsman. The philosopher might well have a better sense of what should be made and, indeed, what the object made *really* is. But with respect to the actual fashioning and reshaping of raw materials and the selection of appropriate tools and techniques for performing such work, the skills of the experienced, well-trained craftsman would be much superior.

It is important, of course, not to treat the concept of *technē* anachronistically. Ellul has argued persuasively that there are substantial differences between the classical notion of art, craft, or skill and such modern notions as technique and formal-instrumental rationality (Ellul 1964, chap. 2). For our purposes—as for Arendt, Foster, Bambrough, and others—it is enough to think of *technē* as a matter of production or fabrication based on specifiable and teachable rules or procedures. And again, it seems clear that this is, for Plato, not a philosophical activity.

To consult Plato with respect to the concept of ruling, one must decide whether his ruler is a craftsman or a philosopher; hence, one must determine how it is that perspicacious readers have come down on either side of this question. More specifically, it appears that one must inquire into the concept of a guardian *(phylax)* as it reflects—or perhaps fails to reflect—the concept of a philosopher (*philosophos*). To pursue such an inquiry, I examine briefly the nature of the cities in the *Republic* where we find and fail to find guardians and philosopher-kings.

The quest for the concept of justice commences, not implausibly, with an attempt to define civic justice; and this leads very quickly to a description of the just city. We may describe this city as a natural and more or less spontaneous ordering of crafts. Among these crafts will be farming, carpentry, weaving, shepherding, trading, and the like. Each such craft is to be undertaken by experts who are, for a variety of reasons, well-suited to performing their tasks effectively and professionally (370c).[2] The city is just only to the degree that these tasks are performed properly, which requires that they be performed by those whose talent and training is appropriate. It seems further that a brief description of these crafts and their interrelations satisfies Socrates as a relatively complete account of the just city. Hence, we may think of the just city as fundamentally a city of experts (Annas 1981, 74–75).

This ordering of crafts appears to emerge naturally and spontaneously, for it is said to be rooted in basic human needs and in the nature of humans themselves as they pursue those needs (369c). Humans must have food and shelter and many other things besides; and their nature is such that none is self-sufficient. In order to provide for their various needs some require the help of others, who, of course, require their help in turn. A talented seamstress, all thumbs when it comes to hammers and nails and boards, is dependent on the skillful carpenter who never learned to sew. Socrates seems to suggest that humans, in seeking to fulfill their needs to the extent possible and desirable, will naturally develop among themselves a web of on-going, mutual, cooperative activities and relationships. One may infer that as individuals become increasingly dependent on one another, this web of relationships will solidify, ultimately manifesting itself as a series of established practices or interactive patterns that together may be said to comprise the structure of a community.

Again, these practices—and hence the community itself—are rooted in the natural coming together of various crafts. As a result, the city is not something made, not the produce of artifice; it simply comes into being. It does not arise out of a plan or architectonic; nor, indeed, can it plausibly be attributed to a clear intention. Though a rational ordering of crafts, it is not itself the product of craft. Indeed, it seems that there is no ruler in this city. At the very least, one can say that Socrates appears to be quite content with a description of it that contains not one word about ruling, law, or political authority in the usual sense.

Of course, Glaucon is not very satisfied with this city and refers to it as a city of pigs, a city devoid of the luxuries that humans, as opposed to animals, typically crave. Socrates nonetheless insists that this first city is the "true city" or "healthy city" (*alethine polis* or *hyges polis*) (372e); and (though there is a long-standing controversy about this) there in fact seems to be no reason not to take him at his word (Strauss 1964, 95–96). Indeed, I hazard the hypothesis that this city is in some sense a model for all cities. As a perfectly harmonious and orderly concatenation of human crafts, it provides its citizens with a life of peace, prosperity, and happiness (372d).

If this is correct, only in the light of this city can one come to terms with the *kallipolis* described in the middle books of the *Republic.* As is well known, Glaucon insists that Socrates describe a just city that is also filled with recognizable individuals. Humans as we know them are apt to multiply their desires far beyond what is natural and necessary. Not content with the simple and prudent felicities of the first city, they seek to surround themselves with luxuries. Instead of wholesome "boiled onions and greens" they demand elegant relishes and desserts to be served them as they recline on low couches; rather than enjoying "sweet intercourse" with one another, they will have courtesans. Their city will be filled with perfume and art and theater; and it will, as a result, "be swollen with a multitude of things which are not in cities because of necessity" (373b). As Socrates would have it, Glaucon wishes to examine a "feverish city," a sick city very far removed from the true or healthy city.

At the core of the *Republic,* then, is an effort to purge (in thought) this feverish city of its excesses, to restructure it to approximate the health and orderliness of the first city. The result of this effort, of course, is the concept of the *kallipolis* itself, as outlined especially in books 3–5. The details of this "admirable city" are far too well known to merit discussion here. For our purposes, it is enough to note that it strongly resembles the city of pigs in being a well-ordered structure of crafts designed to provide for a harmonious and felicitous life; that is to say, the virtue of the *kallipolis,* like that of the first city, lies in how well its parts fit together into an organic whole. And the parts are understood to be the various activities by which humans obtain the things necessary for living a good life.

I have gone over this rather well-worked territory primarily to emphasize that the goal or end of the first city is order and harmony, and that this end is a fundamental part of the definition of the city and applies equally to the *kallipolis.* Other valuable things—prosperity, freedom, virtue—may be helpful in promoting that end in the *kallipolis* but are only secondary.

The similarity between the two cities is thus quite substantial and perhaps helps reveal the degree to which the first provides a model for the second. But the similarities seem to stop here; for although civic excellence continues to be a matter of activities properly ordered, the special circumstance of the *kallipolis*—that it emerges out of, and is expressly designed to overcome, the excesses of *fever*—requires certain quite distinctive features. Specifically, the "parts" or activities of which it is composed are understood very differently from those of the first city. We may say that the city of pigs is made up of various professions linked directly with the actual production of necessary or desirable things; the *kallipolis,* on the other hand, is composed, in part, of activities only indirectly connected with such production, activities of a supportive or managerial nature. I have in mind, of course, functions associated with the class of auxiliaries, who will protect the city from physical threats both external and internal, and with the class of guardians proper, who will manage the activities of all others to preserve civic order. These managerial roles are evidently necessitated by the fact—or persistent threat—of *disease* or *fever.* The various parts of the first city, the healthy city, can function more or less on their own without presenting any danger to civic equilibrium; there is no need for auxiliaries or guardians. But when many of the parts are themselves riddled with disease or have the potential for disease, the health of the polity greatly depends on the ability to cure or at least contain the fever deliberately.

There is every reason to believe that the directly productive crafts of the first city will continue to exist in the second city; and it seems certain that in both cases, felicity depends on the correct ordering of such crafts. The problem of feverishness is so fundamental, however, that in rendering an account of the *kallipolis,* Plato's focus is understandably drawn away from this first structural principle and toward the correct ordering of managerial functions. In other words, the structure of the second city is, by strong implication, twofold: (1) the ordering of crafts involved directly in production receives virtually no systematic mention in books 3–5 of the *Republic,* but it is hard to imagine that Plato did not intend such an ordering; (2) these directly productive crafts are juxtaposed with the support and managerial activities necessitated by the fact or threat of fever. The second aspect distinguishes the second city from the first, and here lies Plato's focus, which, of course, ultimately manifests itself in the threefold structure of the *kallipolis*—producers, auxiliaries, and guardians.

Unlike the first city, then, the *kallipolis* is fundamentally characterized by the presence of a ruling element. Among other things, this means that many of the regularities governing social life in such a city arise not naturally or spontaneously but from conscious human deliberation and choice. Such deliberation and choice is the task of the guardians. I am now in a position to begin asking whether their activity is basically craftlike or philosophical.

The above sketch provides strong reasons, I think, to accept the craft interpretation of Platonic ruling. To begin with a negative claim, it is hard to see why the *kallipolis* would have any need for philosophic, as opposed to craftsmanlike, rulers. According to Plato, it is the business of the philosopher to pursue truth by contemplating the forms or essences of things. This activity, if undertaken assiduously and with talent, may provide the philosopher with something that no one else has, namely, knowledge of the truth rather than simply right opinion. Nonphilosophers are able to act solely on the basis of opinion; and while they may well possess right opinion (according with the truth) this cannot be equated with the possession of knowledge. When a nonphilosopher regards an opinion as right it is a matter of faith, not rational certainty.

But right opinion seems to be all that is required of the guardians. Imagine a master craftsman of chairs. There is no reason to believe that as a craftsman this person has philosophical insight into the form for chair. I assume, nonetheless, that with some strong intuition of that form, the master craftsman's opinion about the nature of chairs happens to be right opinion. The master craftsman may have received this opinion from a philosopher or may simply have chanced on it. In neither case will the master craftsman know with any certainty that the opinion is right but will simply believe it to be so. But if the level of craftsmanship is indeed exemplary and the appropriate raw materials are available, the master craftsman will certainly be able to make excellent chairs.

The guardians need to be rulers only in this craftlike sense. Their function is to arrange and manage the various productive and supportive activities that comprise the social life of the city. Those activities are the raw material, and the guardians need only possess (1) the requisite skills for arranging and managing the raw material and (2) right opinion as to the nature of the city itself so that the raw material will be managed according to the appropriate model.

Qualification 1 is provided by the system of education described in books 2-3, whereby managerial talent is identified, nurtured, tested, and finally certified and empowered. As for qualification 2—having right opinion—this is implicit in the very job description, so to speak, of the guardians. Specifically, the defining function of the guardians is to preserve the orderliness or harmony of the city; this function cannot be performed without right opinion. Therefore, the guardians, as preservers of orderliness, by definition possess right opinion. But there is nothing in this job description (of the tasks the guardians are to perform) that demands anything more from them. Most importantly, it is not their task to deduce the concept of the city rationally; hence it is not a necessary part of their function to have a philosophical understanding of it.

Perhaps it would be useful if the guardians were also philosophers, since this would make their possession of the truth more secure. Rather than right opinion, they would have knowledge. There might be some practical merit in such a proposal, but in principle it would be utterly superfluous since, again, the guardians by definition do possess right opinion. As craftsmen, skilled in the techniques of shaping and maintaining the structure of the body politic, there is simply no need for them to have philosophical insight into the truth. Moreover, just as the philosophical chairmaker is not in any obvious way better able to use tools and raw materials than the chairmaker with right opinion, so too with philosophers and guardians. Indeed, a philosopher who sometimes made things (chairs or laws, for example) would be unable to do so as well as a highly trained craftsman.

Thus, the guardians in principle need not be philosophers; their work seems to require only that they possess right opinion. But can the rulers be coherently understood as craftsmen? As is well known, Bambrough, while acknowledging that Plato adopts the craft theory of ruling, denies that such a theory can make sense (Bambrough 1956). On his account, the craft analogy breaks down in that the craftsman is skilled only in the selection of means, not ends, whereas the ruler must select the ends themselves. Since Bambrough denies that there can be any craftlike knowledge of ends, ruling cannot be a craft. Now Bambrough's outright rejection of moral realism is most controversial. But even if one fully concedes it, his argument still seems to have no force against Plato; for as we have seen, the end of the *kallipolis* is given in advance, as a part of the very concept of the city. Once again, that end, or destination (to use Bambrough's word), is order and harmony, the qualities of the healthy city. But if so—if the goal is a fundamental and presupposed feature of the city itself—we are forced to say that it is no part of the guardian's task to invent or otherwise select that goal. The goal is something that can only be "inherited." Hence, the guardian's function is just to pursue the policies most likely to achieve the given end; and that function involves simply and solely a knowledge of means, a craftlike knowledge.

All of this ultimately suggests even stronger reasons to accept the craft interpretation of ruling. The task of philosophy is to uncover and analyze ideas such as that of the polis. But this in fact cannot be the guardians' task. For the idea of the polis is something already possessed; it must be available before the guardians become guardians. It manifests itself in the particular structure of the *kallipolis*, which includes, of course, the institution of guardianship. Hence, the city's structure cannot be created by the activity of the guardians. Rather, it is the other way around; their activity emerges out of the structure. It is, therefore, unthinkable that guardians as guardians could uncover, analyze, and prescribe the structure of the *kallipolis* since they cannot be guardians until that structure is already fully in place. The guardians can have, then, only one conceivable function, namely, to arrange and manage the parts of the city so that it continues to conform to the idea. And, once more, there seems to be no reason the guardians must have philosophical knowledge of the city to perform this function well; as in the case of the chairmaker, right opinion will suffice.

This account, based on the logical implications of Plato's argument, may suggest not only that the craft interpretation has merit but also that guardians and philosopher-kings are conceptually quite distinct. Such a reading will certainly seem eccentric; but it has, in fact, substantial and quite direct textual support.[3]

The concept of the philosopher-king is not introduced by Plato until rather late in the game, as is well known, but the implications of this may be underappreciated. There are at least two aspects about the initial discussion of the philosopher-king that raise serious questions of interpretation: (1) it is introduced only in connection with how the *kallipolis* might come into being in our empirical shadow-world and (2) the *kallipolis* is described as if it were complete without the philosopher-king.

The entire treatment of the philosopher-king occurs with reference to a particular issue, namely, the possibility of the *kallipolis'* actually coming into empirical existence (Campbell 1980, 374; Martin 1981, 11-12). This is the famous "third wave" of book 5 (472a). Glaucon appears to doubt whether what Socrates has described is possible and challenges Socrates to prove it. Socrates' response to this challenge introduces for the first time the concept of a philosopher-king. Specifically, Socrates himself phrases the question as follows: "We must try to find out and demonstrate what is badly *(kakos)* done in cities today, and thereby prevents them from being governed in this way, and with what smallest change *(smikrotatou metabalontos)*—preferably one, if not, two and, if not, the fewest in number and the smallest in impact *(smikrotatōn tēn dynamin)*—a city would come to be this kind of polity. . . . With one—not, however, a small or an easy one, but possible—we can, in my opinion, show that it would be transformed" (473b–c). The one change Socrates has in mind is, of course, to make philosophers kings or kings philosophers (473d). The implication of this passage is quite clear. The concept of the philosopher-king is attendant to a particular and quite specific reform that, in turn, is to be applied to empirically existing cities so that they can best approximate the ideal of the *kallipolis*. In these passages, there appears to be no indication that the philosopher-king is a part of the *kallipolis* itself. The *kallipolis* is a model, and the philosopher-king is a particular device on the basis of which the model can be approximated more or less satisfactorily in the empirical world.

One might well wonder how philosopher-kings—lacking the skills of craftsmen—could be useful in reforming existing cities. The answer seems quite manifest. Existing cities operate without a clear appreciation of the end they should be pursuing (order and harmony). They may well be blessed with leaders skilled in the techniques of politics, but they remain uncertain of the basic purpose and function of the city to which all other policies should be referred. This, indeed, is their primary defect. Philosopher-kings, having genuine knowledge of the *kallipolis*, would be uniquely and exclusively qualified to prescribe to empirical cities the appropriate goals of civic life and hence, would play a vital role in enabling such cities to pursue the ideal as far as possible. As has been shown, such a role is unnecessary for the *kallipolis*, since it is, by definition, a city that pursues the maximum order and harmony.

We should note with particular care the nature of Plato's project here. Strictly speaking, the question of the *kallipolis*' actually "existing" is necessarily moot. For Plato, ideas have substantiality; the empirical world is, in some sense, a shadow-world. Hence, the *kallipolis*, insofar as it has been properly described, most certainly does exist. Glaucon's concern has to do only with the possibility of getting empirical cities to operate according to the pattern established by the idea. And again, it is in the light of this concern—involving not the concept of the *kallipolis* but only modeling empirical cities according to it—that one first encounters the notion of the philosopher-king.

It is also important that the project of discovering or reconstructing the idea of the *kallipolis* seems to be quite complete prior to any discussion of philosopher-kings. In a famous analogy, Socrates likens this project to painting a picture: "It was, therefore, for the sake of a model *(paradeigmatos)* that we were seeking the nature of justice and of the perfectly just man. . . . We were not seeking them for the sake of proving that it's possible for these things to be realized. . . . Would a painter be any less good who draws a model of what the most beautiful human being would be like and renders everything in the picture adequately, but can't prove that it's also possible that such a human being could ever have existed? . . . Weren't we, as we assert, also making a model in speech of a good city *(paradeigma epoioumen logō agathēs* *poleōs)*?" (472c–e). This describes not only the project of discovering the idea for the city. It also implies—and Glaucon concedes—that the project has been successfully completed. The painting is finished. It presents a model of the ideal city and "renders everything in the picture adequately." Again, this claim is made—hence the model is perfected—*prior* to any mention of philosopher-kings. But if so, the philosopher-king cannot be part of the picture. Rather, as shown above, the concept of the philosopher-king arises with respect to an entirely new and quite different project. It is the project not of discovering the idea of the city but rather of trying to determine how empirically existing cities can be most practicably reformed to reflect that idea best.

At least one author, John Gould, does acknowledge the "odd sequence" of exposition in the *Republic;* but he accounts for it with the curious view that "Plato is setting out his ideas in reverse order to that in which he thought them" (1955, 148n; see also Murphy 1951, 79). Gould says that the third wave of book 5 is a mere pretense and that in the passages in question Plato is "disguising his trail" or "covering up his meaning" (pp. 154–55). Exactly how Gould knows what order Plato thought his thoughts is unclear. As to Plato's meaning, it in fact seems quite plain. The odd sequence is not odd at all if one attends to the eminently plausible explanation given by Socrates himself: the notion of the philosopher-king arises with respect not to the concept of the *kallipolis* but to the project of reforming existing cities.

Indeed, this passage calls into question the well-established tendency unreflectively to regard guardian and philosopher-king as simply synonyms (Barker 1961, 197; Gould 1955, 146–49; Grube 1958, 266–73; Hare 1982, 59–60; and Shorey 1960, 11, 26–27). They seem, in fact, to be quite different things. Guardians—the rulers of the *kallipolis*—are craftsmen and have (for lack of a better phrase) "craft knowledge," that is, merely right opinion. Philosopher-kings—those responsible for reforming empirical cities—are philosophers and have philosophical knowledge, that is, genuine knowledge of the truth. At this point, at least two objections will occur to any reader of the *Republic.* First, it will be argued that in book 3 the guardians are at several points described as "philosophic" (e.g., 412a). This is, of course, quite true, but it seems not to affect the argument in the least. In all such passages, it is clear that Plato intends the guardians to have, at least by comparison with others, philosophic natures. But to have a philosophic nature, to be capable of philosophy, is hardly the same as being a philosopher, properly understood. This is evident, for example, in Plato's discussion of the sophist: "Concerning any seed or thing that grows *(phytou)* . . . we know that the more vigorous *(erhōmenesteron)* it is, the more it is lacking in its own proper virtues when it doesn't get the food, climate, or habitat suitable to it. For surely bad is more opposed to good than to what is not good. . . . Won't we say for souls too . . . that, similarly, those with the best natures become especially bad when they get bad instruction?" (491d–e). Sophists (and perhaps others) may well have philosophic natures, but in virtue of their education and background they turn out to be quite different from philosophers. Thus, the fact that the guardians of the *kallipolis* are those whose natures are most philosophic in no way shows that they are philosophers. Rather, being a philosopher seems to require being trained as a philosopher, and the guardians are trained quite differently.

Second, it will be objected that at one point Plato does explicitly link the guardians and philosopher-kings. In book 6, Plato writes, "Now that this discussion has with some difficulty reached an end, we must next discuss what remains—in what way and by means of what studies and practices the saviors (*soteres)* will take their place within our polity and at what ages each will take up each study. . . . My friend, I shrank from saying what has now been dared anyhow. . . . And let us now dare to say it: philosophers must be established as the most precise guardians *(akribestatous phylakas)*" (502d–503b).

This passage is entirely unambiguous. The guardians of the *kallipolis* are clearly understood to be philosopher-kings. The painting of the ideal city may be thought to have been complete, but on inspection it is now seen as needing some touching up. The original account of the rulers was deficient, and must be fleshed out in the light of the idea of the philosopher.

Thus, there can be no doubt that the guardians are also to be philosopher-kings. Still, the circumstance in which this connection is made virtually forces us to attend to their profound conceptual differences. The passage just cited is the very first indication that the guardians might be thought of as philosophers-kings. Moreover, the notion of the philosopher-king does not emerge originally from a *philosophical analysis* of the just city. Rather, it appears essentially as an afterthought, an idea generated in a different context as perhaps useful (for reasons never made wholly manifest), to be imported back into the picture of the *kallipolis.*

Indeed, even after the introduction of philosopher-kings, it is never made clear why they must be the rulers of the ideal city. This is a serious gap in Plato's argument. The *justification* of philosophic rule is presented entirely with respect to reforming empirically existing cities. One may well concede the value of philosopher-kings in that regard. But this, in and of itself, has nothing to do with the concept of the *kallipolis* as a model, and the transformation (such, in effect, it is) of guardians into philosopher-kings appears to add little to the integrity of the model.

Perhaps the most plausible reading of Plato's argument is simply that the discussion of the possibility of the just city prompts certain discoveries about rulers, which, in turn, suggest emendations in the account of ruling as developed in the original picture of the *kallipolis.* If so, Plato does indeed offer two quite different accounts of ruling. The first, characteristic of the *kallipolis* as originally drawn, is connected with the guardians and is essentially a matter of *technē.* The second, imported into the *kallipolis* from the outside, is a matter of philosophy. It may turn out to be desirable for the same persons to be guardians and philosopher-kings. But guardians and philosopher-kings are conceptually two different kinds of creatures and should be thought to represent the different understandings of rulership available to political theory.

There is, however, a further and extremely grave problem with such an account. For the question of reforming empirically existing cities is undertaken and resolved in the light of the *kallipolis* as originally drawn, in which, again, there is no mention of philosophical rule. Presumably, the correctness of Plato's proposal for reform (to have philosopher-kings) depends on the soundness of the model on which that reform is based, namely, the *kallipolis.* If the model were unsound in some key respect, it is hard to see how it could be a reliable source of reforms. Yet in the context of pursuing those reforms, the model itself must be changed in a very substantial way; specifically, the ruling element will now be construed quite differently, as a matter of philosophy rather than *technē.* By implication, then, the original model, as originally formulated, was deeply flawed or inadequate; but this inadequacy is seen only in the light of a reform thought to be reliable only because the original model was thought to be reliable. The result is something of a paradox. If the original model was not sound, why should we believe that the reform is sound? And if the reform is not sound, why should it inspire a redrawing of the model?

Our most plausible reading, that the concept of philosopher-king is simply read back into the *kallipolis* after the fact, in no way addresses this paradox. Thus, Plato's views on ruling are at best deeply ambiguous, at worst fallacious. The guardians are thought to be philosophers, but the precise reasons for this are never made fully clear. The argument for philosophic rule first emerges not out of the analysis of the *kallipolis* but rather with respect to reforming empirical cities. The quite belated, retrospective introduction of philosopher-kings back into the *kallipolis* is itself never defended and, indeed, now seems to issue in a logical paradox. To leave the matter here, then, would be to raise serious questions concerning both the cogency and coherence of Plato's views on ruling.

In fact, however, the situation is rather worse than this. For there are good reasons to believe not only that guardian and philosopher-king are conceptually distinct, but also that the same person *cannot* be both. Plato's clear assertion, that the guardians of the *kallipolis* should also be philosophers, is in fact a logical impossibility. In order to see why this might be so, it will be necessary to consider briefly three topics that are central to Plato's general argument: the education of the rulers, the character of the rulers, and the question of lying in politics.

Education

As is well known, the *Republic* makes a signal contribution to the Western philosophy of education. But it actually presents two distinct educational protocols, one for the rulers as guardians, the other for the rulers as philosopher-kings.

The first of these takes up a substantial part of books 2–3 of the *Republic* and presents education in what today might be called a behavioral light. The goal of education here is essentially to provide a context in which students can develop sound habits and dispositions. In this respect Socrates makes his famous analogy to the training of dogs. A well-trained dog must be educated to react angrily to strangers but with warmth to someone it knows (376a–b). Such a dog "learns" to distinguish friends from enemies and is, for that reason, "a lover of learning." Socrates wants to say that this is similar to, and an important part of, the education of the guardians. They too must be trained easily to distinguish what deserves friendship from what deserves enmity.

The entire educational experience of the guardians, involving some of Plato's most famous and notorious claims, seems to be aimed at, and well suited to, achieving this fundamental goal. Most important here, of course, is the systematic program of censorship, wherein the young are exposed only to stories and images that are likely to instill good habits. We know that the gods are fickle, occasionally deceitful, often intemperate and impulsive. But these facts must be purged from the curriculum lest they undermine the basic purposes of guardian training. If exposed only to tales of godly heroism, loyalty, and moderation, students are most likely to emulate these traits and to assume without question that behavior based on them is desirable and correct. Thus, the education of the guardians will consist in large part of stories designed to develop and nurture a disposition to behave with spirit, temperance, and fidelity (377). Added to this, of course, will be exposure to the proper kind of music, training in rhetoric, physical culture, a sound diet, and moral instruction—all similarly aimed at establishing habits of decorum and moderation, a kind of high-level obedience school.

In the light of Platonic doctrine, this

program of education raises at least two serious problems. To begin with, the apparent goal—training guardians to treat enemies with hostility and friends with warmth—seems virtually identical to an account of justice suggested earlier by Polemarchus and rejected quite decisively by Socrates. In book 1, Polemarchus had offered the view that justice was a matter of "doing good to friends and harm to enemies" (332d). But Socrates showed that an individual who behaved this way would be unjust, at least in part because it cannot be right for a just man to injure—and hence make less just—anyone else (335d). If this refutation is sound (and there is little in the *Republic* to suggest otherwise) it is hard to see how the rulers as guardians, educated according to the program of the *kallipolis* as originally drawn, could be just. And if they are not just, they must be distinguished sharply from philosopher-kings, whom we know to approximate, more closely than anyone else, the idea or model of the just man.

Second, the initial curriculum seems to contradict fundamental Platonic notions about knowledge and the purposes of education. Such notions are developed at numerous points in Plato's various writings and seem to be unaffected by the changes that we observe otherwise in the transition from the early to middle to later dialogues.[4] Consider, for example, the characteristic arguments of the *Protagoras*, which explicitly examines the question of whether virtue can be taught and in the process addresses the general issue of what it means truly to be educated.

The *Protagoras* begins with a brief discussion between Socrates and young Hippocrates on what it is that one might learn from a great teacher like Protagoras (310b–14c). It is clear that education is nourishment for the soul; but exactly what kind of nourishment this is and how it might best be administered is initially unclear. The bulk of the *Protagoras*, then, may be thought of as a meditation on precisely this question. Protagoras himself early on offers the claim that virtue can most certainly be taught, by which he means essentially that individuals can be trained to obey the law. He offers an elaborate parable about the gods in support of this view, the kind of parable that might well be included in the guardians' curriculum but appears to be of no discursive or philosophic value (320d–23a). He argues further that our belief in the efficacy of punishment as a deterrent provides clear evidence that virtue is teachable (324). And he claims that the traits of the best citizens (rhetorical prowess, for example) are essentially skills that can be acquired through a kind of technical instruction. Protagoras thus offers Hippocrates an education that is at best a variety of technical training and at worst a kind of behavior modification.

Socrates, too, concludes that virtue is teachable, but he means something quite different. Virtue is a matter of knowing the truth in the sense of rationally and self-critically understanding some important features about the world. Such understanding is not a matter of habit or uncritical acceptance but of intellection—using the tools of human reason to evaluate through rigorous argument various kinds of truth claims. Protagoras' student really knows nothing; he has simply been habituated to certain modes of behavior and has acquired certain skills which may be helpful in behaving efficiently. Socrates' student, on the other hand, has the capacity to answer what we might now call, in Kantian terms, *practical* questions—questions pertaining to ends, to the kind of life one ought to live.

Thus, we encounter in the *Protagoras* the distinction between a merely technical education and a "liberal" education, an education in philosophy, broadly construed. It is absolutely clear which of these is to be preferred. Yet in the *Repub-*

lic, the education of the rulers as guardians is exclusively behavioral or technical; only that of the rulers as philosophers, outlined in book 7, is truly Socratic. Indeed, the difference is plainly acknowledged: "Now in our former scheme they (the guardians) were educated by us in gymnastic and music. . . . It educated guardians through the force of habits *(ethesi paideuousa tous phylakas)*—by harmony making them harmonious and by rhythm making them rhythmical—but not by giving them knowledge. And connected with it were certain other habits, akin to these, conveyed by speeches, whether they were tales or speeches of a truer sort. But there was no knowledge in it which would lead to what you are looking for now" (521e–22b). Rulers educated according to the model outlined in books 2–3 will therefore be much different from those educated Socratically. Moreover, this difference is explicitly one of *technē* versus philosophy. For Socrates claims that training for the arts—for *technē*—is necessarily mechanical (522b) and, by implication, that the education of the rulers as philosophers cannot be a technical education. It is, rather, an education in how to think critically, which means training primarily in mathematics, geometry, astronomy, and, ultimately, dialectic.

There is thus a profound difference between these two types of education. But is it not possible for the two to be combined so that rulers will first be given a technical education and then, at a later time, have the opportunity to pursue a more strictly intellectual curriculum? On such an account, Plato might be advocating something similar to Michael Oakeshott's distinction between school education, in which basic skills are learned by rote, and university education, involving critical self-reflection and, ultimately, participation in the "conversation of man" (Oakeshott 1962, 306–15). Perhaps the program described in books 2–3 is really a matter of primary schooling, and the curriculum described later on is to be reserved for the "upper grades." Indeed, this appears to be the standard interpretation (Barker 1961, 198, 216, 219–20, 228–29; Gould 1955, 148–49; Grube 1958, 234–38, 268; Hare 1982, 50–51, 59; Irwin 1977, 201–4; Murphy 1951, 78–79; Nettleship 1929, 79; and Nettleship 1968, 88–93).[5]

There are several problems with such a formulation. First, Plato never states such an intention. The conceptual distinction between the guardians of the city as originally drawn and the philosopher-kings of the touched-up version remains intact, and it is nowhere specified that each ruler will be subject sequentially to two quite different instructional protocols. Indeed, Plato seems quite commited to a rather different view. It is true that very young children will be subject to certain of the "tests" and training regimens outlined in the earlier books and that for a period of perhaps "two or three years" they will learn "compulsory gymnastics" (537b). But he also insists that an education for philosophy should start at a young age: "the study of calculation and geometry and all the preparatory education required for dialectic must be put before them as children" (536d). Moreover, such an education must not be a matter of force or compulsion: "The free man ought not to learn any study slavishly. Forced labors performed by the body don't harm the body, but no forced study abides in a soul" (536e). Plato certainly believes in a sensible curricular sequence. Children cannot themselves be philosophers or successful dialecticians; indeed, young people may be apt to misuse argumentative skills (539b). Nonetheless, from an early age their education is to be intellectual rather than technical, and the goal is to generate a capacity for knowledge and philosophy, rather than merely good habits.

More importantly, the analogy with Oakeshott seems to confuse the learning of basic skills (Oakeshott's "schooling") with a much different kind of educational program, the training of the guardians, which involves learning not simply how to do things but also what kind of person to be. The education of the guardians serves not just to instruct them in the three R's and the like but also to shape their souls and to give them their identity. Indeed, the distinction between Plato's two kinds of education is related to two fundamentally different views of human character. The product of a technical and behavioristic education will be law-abiding and upright in an uncritical way. Students will, for example, know the right thing to do and do it but will not know why it is the right thing to do; they will be well equipped to select the proper means for achieving some specifiable end but will not be able to explain why one end is preferable to another. Their education may be designed so that they will be able to recognize, and act according to, right opinion. But such an education is not simply different from, but incompatible with, an education that eschews opinion and appearance altogether and instead seeks knowledge. A soul directed toward the acceptance of opinion is different from one that aims at—that is *erotically* attached to—knowledge and truth.

This is the entire basis of Socrates' discussion with Hippocrates on the "nourishment" of the soul and involves a fundamental conception of what it means to live the best possible life. The choice Hippocrates faces—to be educated by the sophist Protagoras or the philosopher Socrates—is indeed an either-or. This same choice must be faced with respect to the rulers of the just city; it is impossible to believe that Socrates meant for them to be educated both sophistically and philosophically.

Of recent commentators, only Annas appears to recognize the depth of the problem (Annas 1981; 87-90; but see also Campbell 1980, 366). "Why," she asks, "should people whose early years have been moulded by training in accepting the moral values of their society have preserved the capacity for intellectual rigor and creativity required by these further studies?" She concludes that Plato fails to solve the problem. But this conclusion is correct only insofar as one continues to insist on the identity of guardians and philosopher-kings.

In fact, the education of the guardians would be such as to render them unfit for philosophy; it would turn their souls away from the truth and toward an uncritical and reflexive satisfaction with right opinion. This is one reason, then, why the same individuals cannot be both guardians and philosopher-kings.

Character

Since different educational programs have different implications for the character of individuals, one would expect such differences to be evident in Plato's descriptions of guardians and philosopher-kings. This turns out to be the case. In fact, description of the guardians makes it impossible to believe that they could also be philosophers.

In books 3-5, Plato presents a variety of institutional recommendations pertaining to the structure and function of the guardian class. Perhaps most famous are the discussion of communism and that of the relationship between the sexes; and precisely at these points the character of the guardians as guardians is revealed most clearly. Thus, for example, Plato insists that the guardians should possess no property or money and that, indeed, "for them alone it is not lawful to handle and to touch gold and silver, nor to be under the same roof with it, nor to hang it from their persons, nor to drink from silver or gold vessels" (417a). The guardians are to live a life free from material want but free also from material opportunity, and the reason for this is abundantly clear: they cannot otherwise be trusted. Without a program of austere communism, they will become "masters and enemies instead of allies of other citizens; hating and being hated, plotting and being plotted against, they'll always be in greater fear of the enemies within than those without" (417b). Presumably, the guardians are, in a phrase, all too human—susceptible to the kinds of temptations and unwholesome desires typical of the common citizen. Though trained for leadership, they presumably lack the internal strength to resist the lure of luxury and wealth. As a result, extraordinary institutional precautions are required.

Similarly, the connubial life of the guardians is to be ordered so as to take advantage of, and manage optimally, their erotic dispositions. In outlining some of the circumstances of marriage, Socrates offers predictions such as the following: "And, mixed together in gymnastic exercise and the rest of the training, they'll be led by an internal natural necessity to sexual mixing with one another, I suppose." Glaucon makes Socrates' meaning quite clear: "Not geometrical but erotic necessities" (458d). Elsewhere, Plato suggests institutional devices designed to limit the possibility—and, more importantly, the effects—of incest (461c); recommends using sex as a reward for civic valor and devotion (468c); and prescribes structures designed to ensure that the best men have intercourse as often as possible with the best women (459d). Again, it would seem that the guardians are all too human, that they are possessed of erotic desires at least as strong as anyone else's, and that institutional structures are required to channel those desires in the most socially productive manner. Without such institutional protections, the guardians presumably would lack the internal strength intelligently to control their passion. Exercising naked together in the gymnasium, they would be overcome by eros, gratifying their baser urges to the detriment of the public good.

The character of philosophers as described by Plato is quite different (Strauss 1964, 103, 110–12). With respect to wealth, they are by definition uninterested in it. The philosopher is "in no way a lover of money. Money and the lavish expense that accompanies it are pursued for the sake of things that any other man rather than this one is likely to take seriously" (485e). Similarly, sexual desire appears to have absolutely no hold on philosophers: "they would be concerned with the pleasure of the soul in itself and would forsake those pleasures that come through the body" (485e). It is important to note that these traits or predispositions are utterly internal to the philosopher. They emerge not out of the external circumstances in which philosophers find themselves but rather out of their own natures or characters.

Again, there can be no doubt that Plato regards character as being formed, at least in substantial measure, by educational experiences. But the institutional structures of the *kallipolis* make sense only if we assume that the education of the guardians has failed to make them truly just. They do indeed emerge from their curriculum well prepared to be the political craftsmen of the city, skilled and reliable in applying the lessons of right opinion. But their psyches must be in some sense unbalanced, since extraordinary external safeguards are required to prevent them from indulging the nonrational demands of their souls. The education of philosophers, on the other hand, not only provides them with knowledge as opposed to opinion but also helps to establish in them the kind of character that makes it virtually impossible to imagine that they would be seriously tempted by mere worldly goods and pleasures.

As a result, if the rulers of the *kallipolis* were in fact philosophers, much of the institutional structure of the beautiful city—especially regarding communism and connubiality—would be unnecessary, even absurd. The notion of imposing limits on property ownership would make no sense since philosophers, in virtue of who they are, would not abuse property rights. While Plato would deprive guardians of the opportunity even to touch gold and silver, it seems clear that if anyone can be trusted with money it is the philosopher (i.e., someone whose thoughts and desires are largely bound up with wisdom and truth). Philosophers exercising together would hardly be overcome with physical or sexual passion, since their erotic energies are directly entirely toward the world of ideas.

It is impossible, then, that the guardians could be philosophers. As has been shown, Plato insists that they are. But to allow for this would require, at the very least, dismantling some of the important institutional features of the *kallipolis*. Of course, there is, in principle, no reason why one could not do this. But Plato never contemplates such a step, and it is not hard to imagine why. For the city seems in large measure to be defined precisely by its institutional features. If enough of those features are changed, perhaps it would no longer be the *kallipolis*. If the rulers are understood to be guardians, the picture of the beautiful city seems to be sound; if they are philosophers, Plato would need a new and different city. Plato's both insisting that the rulers should be philosophers and failing to revise the institutional structure of the *kallipolis* is incoherent.

Truth and Falsehood

An important character trait of philosophers is their complete dedication and commitment to the truth. Plato writes, "[They have] no taste for falsehood; that is, they are completely unwilling to admit what's false but hate it, while cherishing the truth" (485c). Indeed, "a man who is really a lover of learning must from youth on strive as intensely as possible for every kind of truth" (485d).

The guardians, on the other hand, appear to have quite an appetite for falsehood. We all know of the famous noble lie or fiction involving the "myth of metals," whereby the people are persuaded that the class structure of the *kallipolis* has a divine basis. But perhaps even more dramatic is the fact that the policies described in book 5 amount to what Socrates admits is a "throng of lies and deceptions" (459d). Emblematic here would the construction of "subtle lots" rigged so as to support an elaborate program of eugenics. Such cases suggest that deceit and falsehood are an integral part of the guardians' rule and that philosophers—*erotically* disposed to the truth and "having no taste at all for falsehood"—would be unable or unwilling to be involved in such activities.

It will be argued, of course, that using deceit as a political tool would be perfectly acceptable to philosophers, provided that the purpose is to benefit the ruled. The philosophers would still be in possession of the truth; the use of lies would be a mere tactical device, which, though perhaps distasteful, would in no way compromise their devotion to knowledge. Indeed, one might claim that the fact that lies can be helpful adjuncts to policy is itself a *truth* which the philosopher can acknowledge, embrace, and act upon (Brickhouse and Smith 1983, 79–96; Cross and Woozley 1964, 103; Guthrie 1975, 475–79; Levinson 1953, 424–49).

The issue is a difficult one. Must lovers of truth always be truthful themselves in dealing with others? But however we decide this, it is clear that philosophers must themselves believe in the truth. Yet it is quite clear that for Plato the *kallipolis* is most perfectly constructed if its rulers, the guardians, believe that which is false. In one of the most astonishing passages of this most astonishing work, Socrates says, "Could we somehow contrive one of those lies that come into being in case of need, of which we were just now speaking, some one noble lie to persuade, in the best case, even the rulers, but if not them, the rest of the city?" (414c). In the best case, then, the rulers should be deceived. They should regard the noble lie as true, thereby subscribing fully to a falsehood. Some authors willfully ignore or even actively deny this fact (e.g., Nettleship 1929, 134). Others acknowledge it but explain it away by saying that Plato's account of the ruler evolves and changes during the course of the *Republic* (e.g., Cross and Woozley 1964, 103). There is, however, no textual evidence to support this latter reading; the initial statement regarding the noble lie is quite categorical, and it is never retracted in the *Republic.* There is, indeed, no clear reason to believe that Plato does not mean it when he says that the guardians ideally should believe in the noble lie. And given Plato's understanding of the philosophic life, of the relationship between knowledge and virtue, of the role played by truth in the structure of the well-ordered psyche, it is impossible to believe that he could prescribe a lie to philosophers, even a noble one. The guardians, then, cannot also be philosophers.

The question of Plato's theory of ruling remains very much open. The *kallipolis* as originally constructed is ruled by craftsmen. But it seems that Plato would prefer philosophical rule. To introduce philosopher-kings into the *kallipolis,* however, would require a substantial reformulation of the *kallipolis* itself. As indicated above, this presents something of a paradox, since the notion of the philosopher-king arises only in the context of a discussion that accepts the original *kallipolis* as the idea of the city, hence as the model against which all cities are to be judged. If the model as originally drawn was deeply flawed, how reliable are the formulations derived from it, including the philosopher-king? There are some, of course, who are quite willing to accept paradox with equanimity, who would perhaps even see a certain wisdom in formulations that resist the usual tests of coherence. But for those who insist on a more orthodox account of what it means to make sense, the existence of such a paradox would be most troubling.

For such individuals, it seems that an extraordinary array of problems would dissolve with the disappearance of the single passage quoted above (502d–3b) that explicitly identifies the guardians and the philosopher-kings as the same persons. Nowhere else is the connection made explicitly and unambiguously. Without this passage, one would be free to regard the guardians simply as the rulers of the *kallipolis,* and the philosopher-kings simply as the proposed rulers of empirically existing cities to be reformed according to the ideal pattern—hence as two quite different kinds of creatures. And if that were the case, one would have much less trouble explaining the basic structure of the *Republic* and the sense in which the *kallipolis* may provide a fruitful pattern for imitation despite the absence therein of philosopher-kings; one would have no great difficulty with Plato's view that the guardians should believe in lies and that they require communism; one could save Plato from the claims of Annas and others that his educational theory is incoherent; one could show against Gould and Murphy that Plato's method of exposition is not at all erratic and account for that method without fanciful reference to Plato's putative hidden intentions; one would have a sound basis for showing why the *Republic* treats rulers as craftsmen (as Bambrough sees it) and also as philosophers (as Irwin sees it); and one would perhaps have something to contribute to the so-called paradox of the philosophers' rule, raised most forcefully by Strauss but pursued by a host of commentators of otherwise varying positions.

There are, of course, no good grounds for eliminating that single passage; nor are there, to my knowledge, clear grounds for thinking of it as an error by Plato, as eligible for radical reinterpretation, as the product of intentional deceit, as an inauthentic "emendation" contributed by some later editor, or the like. Indeed, all we really have is the well-known opening passage of the *Timaeus*, the only place outside of the *Republic* (excepting the related and fragmentary *Critias*) where Plato explicitly discusses the *kallipolis*. In the *Timaeus*—presumably a later dialogue—the description of the ideal city (17c–19a) reflects precisely that of the *Republic* up to and no farther than the third wave of book 5. The just city of the *Timaeus* is, in effect, identical to the painting of the *kallipolis* as completed prior to any consideration of the question of reforming particular cities. If in fact the rulers of the *kallipolis* are to be philosopher-kings, then it is nothing short of astonishing that this feature is not discussed in the *Timaeus*.[6]

It may also be worth noting that Plato's most illustrious commentator views the *kallipolis* in precisely the same way. In book 2 of the *Politics*, Aristotle criticizes several fundamental features of the *Republic's* ideal city—communism, the community of wives, the nature and function of the guardians, and the like. But his picture of the *kallipolis* is perfectly coterminous with that of the *Timaeus*; it stops in the middle of *Republic* book 5, and hence makes absolutely no mention of philosopher-kings. If the guardians of the *kallipolis* were meant to be philosophers, one would guess that Aristotle, at least, would have known about it.

Notes

1. Some scholars would deny the craft-philosophy distinction, at least as here presented (Barker 1961, 218–19; Hall 1978, 169–73; Henwood 1979, 310–12; Reeve 1988, chap. 2; and Tiles 1984, 49–66). For a critique of such views, see Annas 1981, 262–71 and Campbell 1980, 365–67. Annas sees Plato's position as problematic and perhaps contradictory; the present essay is, in part, an attempt to account for this contradiction.

2. I rely on the Jowett-Campbell edition of the *Republic*. In rendering passages in English, I have consulted primarily the work of Bloom, Grube, Jowett, and Lindsay. I am also grateful to Anita Estner for her valuable advice on numerous problems of translation.

3. It was a matter of some astonishment to learn that my colleague David Reeve and I had quite independently come to certain similar conclusions about the differences between guardians and philosopher-kings. Beyond that, our readings of the *Republic* are sharply divergent. But this divergence in no way qualifies my judgment that his book provides a brilliant and quite profound—perhaps even unsurpassed—account of Plato's argument.

4. Compare the ensuing discussion of the *Protagoras* with, for example, *Apology* 24d, *Meno* 91a–93a, *Laches* 179c–d, and *Alcibiades* 110c–11a. For a more general account, see Shorey 1960, 14–15, 18, 21.

5. Again, Gould notes the "haphazardness of exposition with respect to education" (1955, 148n), but explains it away as a mere aberration. Irwin, of course, would deny the continuity between the Socratic teaching of the *Protagoras* and the Platonic teaching of the *Republic*.

6. See Barker's brief, dismissive treatment of this issue (1961, 197n.).

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FREEDOM, RECOGNITION, AND OBLIGATION: A FEMINIST APPROACH TO POLITICAL THEORY

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I argue that a feminist methodology can help liberal political theory get beyond the problems that it has been recycling since the seventeenth century. Taking political obligation as the focus for my analysis, I show how feminist psychoanalytic and psychological theory can help uncover the structural sexism of liberal theory and epistemology and point the way toward more consistent—and less biased—theoretical formulations. Rejecting the essentialist view of gender difference that has been attributed to this literature, I argue that it is more interesting and appropriate to read it as a symbolic language of power and as a heuristic device for uncovering the gendered dimensions of supposedly "neutral" concepts like obligation.

In the modern liberal state and the liberal doctrines that theorize it, it is fairly well established that obligation is a concept based on voluntarist principles; that is, an obligation is a limitation on behavior, a requirement for action or nonaction, that the actor or nonactor has chosen or agreed to. As opposed to duty—which may be a requirement that exists "naturally" (Rawls 1971) or "positionally" (Simmons 1979) but at any rate is not explicitly chosen—it is central to our understanding of obligations that they arise from our voluntary and free actions.[1] The paradigm for obligations on the liberal model is the promise and contract (Hume 1948; Pateman 1985).

The political paradigm is the social contract, which founded the ideal that political obligations are based on the free choices of citizens, on their "consent" to a government's authority. The reasoning of the social contract theorists was that if people are inherently, naturally, and (most importantly) equally free, how could any limitation on that freedom be imposed on an individual by another individual or a group? The only legitimate limitations are those imposed by the self; for if I am above all else free, a limitation of freedom can be legitimate only if it is simultaneously an expression of my freedom. Thus, the social contract is an expression of human freedom, of our ability to make choices and control our destiny. Of course, all of the social contract theorists imposed limits on people's ability to consent (one could not consent to die or to be a slave or to do evil); but these limits, it was argued, only served to define—rather than further limit—freedom.

This theoretical framework of the priority of liberty carries over into contemporary obligation theory. Many of these theories overtly adopt consent as the basis of obligation but transform how consent is expressed and even its very meaning. For instance, in attempting to reconcile the centrality of consent to obligation in the face of the fact that many people do not even think about their consent, Tussman (1960) suggests assuming that the majority of citizens give their consent like "political child brides," who let others protect and define their best interests. Pitkin (1965), through a creative reading of Locke, develops the notion of "hypothetical" consent: what perfectly free, rational beings would consent to determines what the average unfree, unequal person is obligated to. Flathman (1972) argues for a "good reasons" approach, holding that critical reasoning will reveal that we have (or do not have) good reasons for respecting the authority of a government; while obligation is a "practice" and must operate within the confines of language (this automatically limits what can count as a reason), the final repository is individuals' ability to judge and decide for themselves, one of the main problems that consent theory raises. Even Pateman (1985), who is highly critical of liberal obligation theory, argues for a "fully consistent" consent theory, which can be realized only in a Rousseauist participatory democracy.

Theories ostensibly challenging the idea of consent as the basis for obligation turn around and develop "alternative" theories of obligation that take as their cornerstone the same framework and assumptions that ground consent theory (and indeed, make consent the only possible basis for consent), namely, the basic priority of individual liberty and the necessity for active and individual choice. For instance, Rawls's "fair play" principle depends on the active acceptance of benefits within a cooperative scheme; while a context of justice is important, passive acceptance or nonavoidance of benefits does not generate obligations. This is juxtaposed to his concept of duty, which is "natural" and not the product of choice. Both duty and obligation involve "schemes" or ends that are just; but obligation, to be such, must be truly voluntary. However, within Rawls's structure, if one does not accept the benefits of a just scheme and hence has no obligation to it, one may still have a natural duty to it by virtue of its "justice." If it is just, one would have agreed to it in the original position. This fact calls into question the usefulness of Rawls's distinction between obligation and duty and highlights a blind acceptance of voluntarism as a prerequisite for obligation even when it does not make theoretical sense.

The problems with voluntarist conceptions of obligation, as numerous theorists have pointed out, are manifold. Since Hume, critics have rejected consent theory on descriptive grounds: When have we ever had a society where the citizens actually were able to give consent and did so? It is also rejected, however, because of internal contradictions and inconsistencies. Some contemporary theorists, such as Simmons (1979), have concluded that obligations simply do not exist. Others, like Pateman (1985), hold that it is not obligation or consent per se but their specifically liberal formulation that is to blame.

None of these critics goes deeply enough to articulate the *reasons* for the problems obligation theory displays. They address themselves to the symptoms, not the causes—with the predictable result that the exact same problems recur, albeit in altered form, in the "new" theories developed to replace the social contract. Even Pateman, who goes farther than many other theorists in tracing the problems of consent to a source, namely, abstract individualism, leaves unasked the question why abstract individualism is so important to these theories.

The answer to this question is extremely important for getting beyond the problems of liberal obligation theory to the creation of a truly new theory, which is my intent here. I assert that the inconsistencies found in voluntarist theories of obligation are inevitable and irresolvable at least in part because of a particular epistemological bias, namely, gender bias. Here I examine one particular avenue of that suggestion, exploring whether a feminist analysis can uncover the deeper roots of the problem of obligation. I will argue that the gender bias found in liberal obligation theory is not merely contingent but structural as well.

By contingent gender bias I mean cultural biases against women, which deny women opportunities for consent and pervert consent theory as a result. Practices of excluding women from standard means of consenting and of denying women the opportunities for choice afforded men present a contradiction to consent theory that it should and can rectify. A fully consistent consent theory would allow women full opportunities to choose their obligations.

This type of argument is not uncommon in political theory and in feminist politics (Jaggar 1983; Pateman 1980). But few have recognized the deeper, structural components of this exclusion. By structural gender bias I mean the bias of the very structure of obligation (its being defined solely in voluntarist terms, and the fact that *nonvoluntary obligation* is an oxymoron) toward a masculinist perspective which automatically excludes women from obligation on an epistemological level. In this light, the provision of "full opportunities" for consent begs, rather than answers, the question of obligation. By declaring that all obligations, to be such, must be taken on voluntarily, consent theory ignores or denies what women's experience reveals, namely, that obligations do in fact exist that are not chosen but stem from the history and character of human relationships. A fully consistent consent theory would have to include (perhaps paradoxically) the recognition that not all obligations are self-assumed.

Object Relations and the Standpoint Approach

To establish this claim, I adopt an approach suggested by Hartsock (1983) called "the feminist standpoint." The feminist standpoint is a variation on the more general conceptualization of Marxian "standpoint epistemology," which was specifically formulated as "the standpoint of the proletariat." A standpoint is the perspective from which one views the world, social relations, and hence reality. It is composed of factors such as race, class, gender, and the kind of work one does; I add psychosexual development. The standpoint approach holds that different people will develop different knowledge frameworks depending on their experiences and circumstances. These experiences form a crucible for perception, interpretation, and understanding by moving many of the unconscious aspects of experience into the realm of consciousness. To the degree that a particular group of people (e.g., women) share socially and politically significant characteristics, they will share a standpoint. Since "epistemology grows in a complex and contradictory way from material life," the standpoint "structures epistemology in a particular way" (Hartsock 1983) that reflects experience.

This does not mean that the feminist standpoint is natural to all women, however; it is not a mere unconscious bias but must be "achieved" or at least acknowledged. This is where political theory is important; it can help translate experience into political meaning or articulate the political significance of women's experience. Feminist standpoint epistemology rejects the idea that epistemology is objective or universal; it holds that epistemology is itself a product of particular social relations. Not just knowledge (*what* we know) is shaped by particular experience and the relations we have to others, but so are *how* we know and how we conceive of knowledge. Thus, if experience differs among groups of people, their epistemological orientations will differ as well.

But it is not just "difference" that is at issue; the standpoint approach further holds that the standpoint of oppressed groups enables them to see more aspects of the social relations that oppress them. Thus, just as the proletariat has a potentially superior vantage point from which to understand the relationship between worker and captialist, women have greater potential to understand more fully the relationship between men and women. This might suggest that the feminist standpoint is gender-exclusive, but Hartsock would not agree. She specifically uses the term *feminist* rather than *female* to point out both "the achieved character of a standpoint and that a standpoint by definition carries a liberatory potential" (Hartsock 1983, 289). The process of "feminist struggle" welcomes male participants. Other theorists, (e.g., Harding [1986]) are very ambivalent about this conclusion—not because they do not want men to be feminists but because they doubt whether the standpoint approach will allow them to participate in "the struggle."

Not surprisingly, there are opponents of the standpoint approach to feminist epistemology, most notably feminist postmodernists. I will not enter the discussion in great detail here (but see DiStefano 1988) except to say that I think the debate is cast in overly dichotomous terms and that I recognize the problems inherent in a concept of *the* feminist standpoint. I will be arguing here for only one of many possible feminist standpoints. The *concept* of a standpoint, no matter how any given standpoint is defined or determined, is philosophically and politically useful and educative in the attempt to understand and analyze mainstream political theory.

The aspect of a possible feminist standpoint that I find most illuminating is offered by object relations theory. This is a school of psychoanalytic theory that maintains that because women, not men, "mother" (that is, have primary if not sole responsibility for the care and nurturance of young children), boys and girls will develop different senses of themselves as gendered subjects as well as different conceptions of their relation to the "object-world," or world outside the self. The power of object relations theory lies in its epistemological implications, for it suggests two very different ways of seeing and "knowing" the world. One fits the dominant discourse of political theory (and political obligation in particular); the other is at odds with it. One speaks the language of rules and rights codified in theory; one challenges that perspective.

Before infants come to have a sense of self (at about six months), their primary identity is subsumed in the one who supplies their needs, who is most generally, across all cultures, female (Rosaldo and Lamphere 1974). Thus, both boys and girls are originally, psychically female. But because gender is culturally an exclusive category, the boy must "become" male. In learning his gender, the boy perceives a fundamental difference between himself and his mother; the girl will learn she is "the same." And because the primary caretaker represents the entire object-world to all infants, the girl comes to see herself as connected to the world: self and other will constitute a continuum for her. In contrast, the boy will come to perceive the self as fundamentally separate and different from the object-world; self and other will be a dichotomy. Becoming male entails making a radical break from primary femininity, represented by the mother, resulting in an overemphasis on separation; a boy defines himself against the mother, as "not-mother."

Such a defensive conceptualization of the self produces what Chodorow (1978) calls "reactive autonomy," a separateness and independence that is a reaction against others. It produces a conception of agency that abstracts individual will (the ability to make choices and act on them) out of the context of the social relationships within which it develops and within which it is exercised, because it sees those relationships as threatening by definition. Furthermore, because the boy's identity is partly formed by the roles that patriarchy dictates for males, the "reaction" is not even to the mother as a person but to what she symbolizes—the boy's primary femininity. Becoming male means becoming "not-female." So while autonomy is defined as independence, its reactive character ensures that others set the terms of one's identity. This contrasts with "relational autonomy" (Bakan 1966), wherein the self derives its strength from its context of relations, not from the absence of others. Thus, "true" autonomy can develop only through a close personal relationship with a mother who is recognized as her own subject (Flax 1978; Mahler 1968); but in mother-only child rearing, this subjectivity is denied. The resulting concept of autonomy as reactive separation is, like consent theory, mythologizing; for autonomy is a highly relational concept, intrinsically tied up with a qualitatively particular relationship.

These contrasting senses of self and resulting views of the world translate into differing "standpoints" and particular moral perspectives. According to Chodorow, girls' greater sense of connectedness with mother and the world means that they have "a basis for 'empathy' built into their primary definition of self in a way that boys do not" (1978, 167). This is supported empirically by Gilligan (1982), Johnstone (1985), and Lyons (1983), whose studies maintain that females perceive themselves as more connected to others while males tend to see themselves as separate. These contrasting perceptions of the self and its relation to the world result in males who reason more in terms of competing individual rights and rules and females who conceive morality in terms of relationships and overlapping responsibilities. Gilligan in particular argues that women's worldview of connectedness results in a morality of care.[2] Thus, object relations theory suggests that by producing gender-related differences in males' and females' sense of themselves and their relation to the world, exclusively female child rearing provides the material basis for a feminist standpoint, a feminist vision of reality.

I am not suggesting that we take this literature as an essentialist statement about how men and women think. While we can take object relations theory as partly empirical hypothesis, it is also useful to view it as a theory of power. Psychology and psychoanalytic theory—in spite of their misogynist history—gives us a language and vocabulary of power that are very useful for political theory and that *can* transcend gender. After all, men such as Martin Luther King (1969) have expressed the voice of connection and care and have been just as marginalized as women. At the same time, that voice has a history, as does obligation. It is not just coincidence that women have been for the most part powerless and expressed the voice of connection. Indeed, the activities of the private sphere to which they have been assigned—child care, nurturance, affection—have required women to draw on and develop that voice. So while this is not exclusively a "woman's voice," there is a loose gender relationship deriving from history, material experience, and socialization as well as psychology. This grounding in women's experience is the whole point of calling it feminist.

But if we use terms like *the boy* or *the girl* as abstractions that idealize and represent relationships of power, object relations can be seen as a heuristic device for uncovering the epistemological gender bias of Western thought. Most importantly, by identifying individual development as in part the product of created institutions, namely, the social relations of child rearing, object relations can translate individual experience into cultural phenomena or at least explain the institutional and cultural aspects of a supposedly individual experience. If the processes of psychic development produce a view of the self as fundamentally separate or fundamentally connected, this will inform one's view of the world, which in turn will shade one's perception and interpretation of truth, or reality. And of course, if such self-conceptions are gender-related, the accompanying world view will differ by gender as well. Thus, this work suggests that the dominant epistemology that defines political concepts such as obligation, as well as the method of our dominant political theoretical discourses, may themselves be an essential locus of gender bias. So object relations can be an important means of understanding how the problems of liberal democratic theory go beyond the empirical exclusion of women from politics to the fact that the epistemology from which these theories operate is *premised* on that exclusion.

The Priority of Liberty

Viewing object relations in this way has great potential for challenging many ideals of liberal obligation theory, such as the public-private dichotomy and the inviolability of the individual; for it suggests that what happens in the private relations of child rearing influences how we maintain and define the public. It further reminds us that individuals are not static entities in a state of nature but dynamic beings created and shaped by a variety of factors, particularly relationships with others.

But the strongest link to obligation is through the notion of liberty. As I suggested earlier, liberal voluntarist theories of obligation operate centrally from a notion of what Berlin (1971) has called "negative liberty," consisting of absence of restraints. The individual is free to the extent that she is not restrained by external forces, primarily viewed as law, physical force, and other overt coercion. As anyone familiar with the debate between positive and negative liberty can attest, a central difficulty with the concept of negative liberty is that of determining what exactly constitutes a restraint (Presston 1984). However, Berlin's general concept that restraints come from outside the self is an important basic tenet of negative liberty; specifically, other humans' direct or indirect participation "in frustrating my wishes" is the relevant criterion in determining restraint: "By being free in this sense I mean not being interfered with by others. The wider the area of noninterference, the wider my freedom" (Berlin 1971, 123).

Note the resonance between this concept of liberty and object relations. The quotes selected particularly echo the boy's infantile dilemma. But first I want to look at the term *liberalism*, held by some to be rather amorphous. It may include views ranging from a Hobbesian rational egoism to utilitarianism to Dworkin's and Rawls's concern with the welfare state. Indeed, it is amazing how much liberalism can continually revise itself and still be called liberalism, a privilege generally denied other ideologies. (Could that be testimony to its hegemonic entrenchment?)

The view of liberalism taken by its stronger critics, including a number of feminists, links liberalism inextricably with abstract individualism (Hartsock 1984; Macpherson 1962; Pateman 1985). But I wish to focus on what is less controversial, namely, that one thing common to the various liberal theories is the priority of liberty over other values. Indeed, the very root of the word *liberalism* derives from liberty; as Cranston (1967, 458) says, "By definition, a liberal is a man [*sic*] who believes in liberty, but because different men at different times have meant different things by liberty, 'liberalism' is correspondingly ambiguous." This, of course, is the positive-negative liberty debate; and it can be argued that the welfare state sort of liberalism in fact adopts a positive libertarian view. Yet I maintain that liberalism adopts a consistently negative, not positive, conception of liberty and that positive libertarians like Rousseau are misdescribed as liberals.

For instance, while Rawls ostensibly takes justice as his focus, this is actually subordinate to, and defined in terms of, a real priority of negative liberty. Justice is established through the lexical ordering of principles that give a priority to liberty over equality. Equal worth of liberty—something we might associate with a positive, political liberalism—must take a back seat to liberty per se. Rawls's seemingly innovative ways of addressing inequality, such as the difference principle, serve to justify inequality by veiled appeals to such negative liberty principles as opportunity. Similarly, his theory of the natural duty of justice, as indicated earlier, is unnecessarily complicated by a voluntarist theory of obligation, in an attempt to preserve voluntarism in a nonvoluntarist construct (see Pateman 1985; Simmons 1979).

It can be argued that theorists like Locke are really not so extremely negative as they have been portrayed. Indeed, Locke asserts that "*Law*, in its true notion, is not so much the Limitation as *the direction of a free and intelligent Agent* to his proper interest" (1964, 347–48), suggesting positive liberty. In this respect Locke may lead the way to a more social view of humans and perhaps of obligation. But he remains firmly in the negative liberty camp. First, Locke also defines liberty as "to follow my own Will in all things, where the Rule prescribes not" (1964, 324), emphasizing the necessity of spheres of action not subject to law. Second, what makes law the direction of my will is the fact that I created it, via consent. Of course, laws of nature are to be obeyed because they come from God, not because of consent per se. But because God makes all men rational and coherent with the divine order, they will all naturally want to obey the laws of nature. For example, even if I am too dim or evil to see that the law of nature prohibiting waste is what God wants, I will at least be able to see that violating it is irrational, in that it wastes my labor. So even laws of nature would seem to be legitimized on some level by consent.

The third reason is the most convincing, if most complex; the very fact that Locke felt compelled to develop the doctrine of tacit consent demonstrates his beliefs concerning liberty. Though Locke binds the average unfree, unequal citizen to the state without explicit free choice, he feels compelled to assert that consent is nonetheless given. Through tacit consent he articulates a myth of choice to legitimize coercion through voluntarist language. This is not likely a conscious deception on Locke's part but rather a sincere attempt to reconcile freedom with authority. But it betrays Locke's agenda, for the only justification for the very awkward ideology of tacit consent is the belief in the need for negative liberty. A positive libertarian would not need such a myth (but might need others, as Rousseau demonstrated). A positive libertarian would simply say, "Obey the law because it is in the interest of your higher will, whether you are aware of that yet or not." Thus, the priority of liberty is demonstrated through the assertion that citizens consent even if Locke must use circuitous (if not circular) reasoning to establish it; and the concept of liberty Locke endorses is decidedly negative.

By confining myself to obligation, I hope to establish even more strongly the claim that freedom is central. Social contract theory begins with the assertion of natural freedom and equality; and its more explicitly liberal versions, such as

Locke's, further define equality to serve primarily as a means to define and protect freedom. The absolute natural freedom of the state of nature outlined by the social contract theorists entails an absence of restraint except for the restraints imposed by competing and alienated others. The absolute freedom of the state of nature—particularly in Hobbes and Rousseau and to a less obvious degree in Locke—is marked by a lack of control. Fear of ceasing to exist, fear of losing one's possessions, hostility, and suspicion are dominant characteristics (Flax 1983). Even for Locke, the state of war inevitably follows from the state of nature, either because money creates rational reasons to violate the laws of nature concerning hoarding (Macpherson 1962) or because there is no known judge (Dunn 1980). The state of nature is a kind of prison; for within it people cannot realize themselves, they cannot create and control. People in the state of nature seek to escape this chaos by turning to the social contract, thereby trading absolute freedom for "effective freedom," the freedom to act with assurance that one's actions will produce desired results.

Civil society thus seeks to protect the natural freedom that is threatened through war and conflict. It does this in two ways. First, civil society protects possessions and property. By having control over posessions, the individual has control over personal identity. For Locke, for instance, who you are is importantly determined by what you have; and civil society is established first and foremost to protect property. Property is central to freedom. Life, liberty, and property are the basic rights God gives to humans; it can even be argued that property subsumes life and liberty in the liberty to contract one's labor in exchange for wages and in the absolute centrality of property in the social contract. The expansiveness of the term property (Locke uses it alternately to refer to land and the body) also indicates its priority. Similarly, for Hobbes, acquisition and the fulfillment of passions is central to his concept of a person. People must be able to acquire *things* (property) to fulfill their natural passions and perpetuate their motion.

But civil society also exists to ensure that the individual can act rationally. Rules and laws provide predictability, assurance that individuals can act with certainty and hence control their lives. By protecting property and preserving (or making possible) rationality, the social contract not only preserves the citizen from nature but also assists the individual's preservation of autonomy, defined reactively as self-control and self-mastery. From the perspective of this reactive autonomy, obligation necessarily exists only by an act of free will. If I am free above all else (if freedom is what makes me human) I can only be bound (I can only have connections and relationships) by an act of my own free agency.

The logic of the contract thus follows inescapably from the premises of the state of nature. The necessity of consent to obligation must logically derive from the freedom of individuals and from the concept of freedom defined negatively. A positive libertarian like Hegel would force people to be free by forcing them to obey and thus would not necessarily require consent for obligation. Also, by focusing on *individual* consent in the state of nature, these theories make consent to greater or lesser degrees a sufficient condition for obligation. If no relations among people are considered natural,[3] they can only be the product of agreements. As Simmons (1979) argues, the tendency of consent theorists to put limits on consent is not a consistent extension of their theories but a contradiction to the core principles underlying consent. The necessity and sufficiency of consent, in turn, rely on a concept of liberty as absence of restraint, because only such a concept is fully compatible with the extremely individualist view of consent and choice endemic to consent theory.

Freedom and Recognition

If we grant that this negative conception of freedom lies at the heart of voluntarist conceptions of obligation, this gives us a richer perspective on the gender bias of liberalism; for it can be seen in important ways to cohere with the symbolic language of "masculine experience" under mother-only child rearing. According to object relations, the girl, perceiving sameness between herself and her mother, incorporates that sameness into her self-definition and view of the world. She views her relationship with the world as *continuous*: other is self. For the boy, perceptions of difference cause him to view self and other as totally separate. And these perceptions feed on themselves as the boy actively engages in the conceptualization of the mother as completely other and outside the self.

In dissociating masculine mind-self from the female body-other, restraint is for the boy embodied in the very presence (i.e., the body) of the mother, as the reminder of the boy's primary femaleness and how it is at odds with his masculine gender identity. The mother is viewed as a controlling force that seeks to keep the son imprisoned, that is, merged with her. Her presence thus presents a "barrier" to self-realization of (nonfeminine) masculinity, a limitation and restriction on the boy's ability to become male, to become "himself." Furthermore, this restraint evident in the mother's presence is seen as coming from completely outside the self. Its genesis seems totally other, in spite of the fact that what he is trying to escape—his psychically female identity—is his primary self.

In reaction, the boy "cuts loose" from the (m)other. He detaches, tries to escape her influence and control. By projecting his psychic femaleness onto the mother and by viewing the mother as completely separate, the boy can dissociate himself from his primary femininity. On this reading, a primary goal of the emerging Oedipal boy is to achieve freedom from the constraint of his mother—to excise his femaleness, detach thoroughly from the mother and be free of the female, and thereby "escape the body-female." In short, he seeks absolute freedom from her and from all "others."

But the "freedom" defined by this reactive autonomy further entails domination and contest. Precisely because the deep nature of the boy's psychic femaleness makes it impossible to truly excise, the mother is viewed as a powerful controlling force, inhibitive of self-realization by virtue of her very presence. The search for freedom thus becomes a struggle for control in the boy's subconscious. If he can dominate his mother, who represents his primary identity, he can master that identity. Thus, the boy devalues the mother and his relation to her, belittling all relationship (i.e., "others") in the process. He seeks to deny her subjectivity, her selfhood, and particularly her sexuality. Thus, this "freedom" is viewed as the product of a struggle; the boy achieves freedom only by subordinating the woman.

In reality, however, this freedom is a false abstraction; for in the effort to escape the restraints embodied in the mother-woman, the boy must erect other, artificial barriers; that is, in order to prevent loss of self to the mother, the boy erects barriers to keep her "out" and him "in" his self-identity and gender identity. These barriers range from socially approved institutions of female-exclusive masculinity (from all-boy sports to male-dominated professions) to more pernicious ones, such as widespread belief systems about women's natural inferiority ("the normal male contempt for women" that analysts document in boys from age five on [Brunswick 1940]). These belief systems are partly produced by empirical observation and teaching. The boy can see that women are socially devalued even by observing relations between mother and father. He experiences privileges over his sisters or his friends' sisters. He observes the restraints placed on women by virtue of their sex. But other beliefs develop by extension of these observations because they fill a deep need in the boy to *believe* that women are inferior; for if they are, his mother's power is perhaps not so threatening after all. These belief systems, rules, and practices serve as barriers—restraints—to prevent the boy's return to the mother. Like Locke's "natural law" preventing men from consenting to slavery, these belief systems and rules serve as restraints on action to the end of preserving freedom.

Thus, while this concept of freedom may ostensibly be defined as an "absence of restraint," the conceptual framework afforded by object relations would suggest that it can also be defined as an "absence of the female." Indeed, the notion that freedom in this sense is the hallmark of humanity provides another means of asserting women's nonhuman status. If women were human, they would have a right to freedom. To enact this freedom they would have to seek dominance; and such a search would destroy men, not to mention the fact that it would subvert the very purpose of defining freedom this way in the first place. As long as women are not considered to be human, freedom is not relevant to their existence. They are inhuman and therefore dominatable (like all things in nature) and therefore dominated. But this domination is what asserts women's inhumanity in the first place.

It goes even deeper than this; for if female is "other," freedom entails the absence of the other. This, as Beauvoir and Hegel both brought into our collective intellectual consciousness, constitutes the problem of recognition. Recognition is a key issue to the negative conception of freedom in the masculine psyche; and it is key to the conception of freedom found in liberal voluntarist theories of obligation.

In *The Phenomenology of Spirit,* particularly in the chapter "Self-Consciousness," Hegel argues that the self seeks affirmation by declaring a radical independence. In this, of course, it requires recognition by another being; but self-consciousness seeks to gain recognition from an other without making a simultaneous recognition. Self-consciousness seeks to be perpetually the self, keeping the other perpetually other. Self-consciousness is prepared to die for this recognition. And even though it realizes that death would bring a dubious (if not Pyrrhic) victory, this readiness is what creates masters. Those not so prepared become the slaves, the perpetual objects. The slave is thus "the dependent consciousness whose essential nature is simply to live or to be for another" (Hegel 1977, 116).

This presents a parallel to the "male model" of pre-Oedipal development. For the boy, recognition of the mother as a subject is "dangerous," for it would entail recognizing and accepting his own femininity. Yet because repressing primary identification and excising the female is so difficult (if not impossible), the need to deny the mother as the embodiment of his own femininity, becomes intense. And the struggle for recognition becomes, in the boy's unconscious, a life-and-death struggle for dominance. The boy thus resorts to the defensive reaction of taking difference—specifically masculinity as "not-female"—as ostensible validation of his separation or differentiation (Flax 1983). This, however, is an artificial solution with mixed results: it "involves an arbitrary boundary creation and an assertion of hyperseparateness to reinforce a lack of security in a person's sense of their [*sic*] self as a separate person" (Chodorow 1979, 56).

Not prepared to die for the ideal and perhaps even oblivious to the struggle itself, the mother-woman becomes the slave. She is a thing, the perpetual object. Furthermore, because of her perspective—one that values the concrete and real over the abstract and ideal and takes relationship as primary—the mother-woman does not readily perceive this as oppression. She cannot conceive of wishing to die for recognition, which seems to her an inherent contradiction. Her realism and relationship orientation, however, ensure her "enslavement" (objectification, dehumanization, oppression). She, like Hegel's slave, is doomed to "live for another." Yet the reason she does so is not the natural outcome of the dialectic but the result of masculine action. Because of the male's intense need for unidirectional recognition, he creates institutions that solidify woman's role as other.

But in keeping with the standpoint approach, just as the master is in many ways in an inferior position to the slave because he is blind to the importance of material life to self-consciousness, so the boy develops a stance that, while keeping him master, also keeps him from a true realization of his self. For the one-sided recognition he achieves through patriarchy is far from the full recognition required for "relational autonomy," or the full autonomy that can only be achieved through the mutual interaction of subjects and of the location of the self in relationships of mutuality. Rather, we have relationships of domination, which "fail to promote mutual recognition because they prevent individuals from seeing others as anything but totally other; and they accomplish this false othering by promoting differences meant to keep individuals on one level of a hierarchy from being able to recognize individuals on different levels" (Aboulafia 1984, 182–83).

Implications for Obligation

What does all this have to do with obligation? I contend that liberal obligation theory has primarily embodied a "reactive" concept of autonomy. The previous discussion has overtly articulated some of the central issues and relevant problems involved in the application of object relations and standpoint epistemology to political theory. It enables us to examine the issue of recognition in a deeper way than liberal obligation theory would at first glance suggest. The themes of the desire for dominance and nonreciprocal recognition have evident parallels in liberal theories, particularly in theories of obligation. They are merely represented more subtly and covertly.

Agency is one of the most important concepts of individualism and consent theory; the individual has the capacity to make choices and thus can assume obligations. But this capacity also carries a moral imperative that obligations can be created *only* by exercising this agency. Agency is the hallmark of independence, autonomy, and adulthood. To be able to make one's own decisions indicates an end to dependence on the will and abilities of another. Hence, agency is what justifies the rejection of divine right and the adoption of the social contract. But because it is a reactive, rather than a relational, autonomy that this agency embodies, it is also what justifies—indeed, creates and perpetuates—the radical and abstract individualism of liberal democratic theory, the market model of society, a theory that obligation can exist only by virtue of voluntary assumption. And these stem in part from the nonrecognition of women.

That is the primary significance of object relations theory for political theory. Because he must become masculine in a world where mother-only child rearing ensures that he is psychically female, where gender is an exclusionary category, and where the female is devalued while the male is elevated, the boy cannot afford to grant recognition to the mother nor, by extension, to any women nor, indeed, to any others. The conception of people as absolutely separate, which the boy develops both from his perception of difference from his mother and from his exaggeration of that difference to bolster his differentiation from her result in a framework in which those separate individuals can resolve conflicts only at discrete and controlled points of contact, only through rule-governed and role-defined structures. This "inability" to grant mutual recognition is the vital seed from which the self-other dichotomy grows, as well as other dualisms that are variations on that theme: subject-object, mind-body, public-private, fact-value, exchange-use. Not coincidentally, these dualisms involve identification of male with the former (the public world of fact, the subject, and the ego) and female with the latter (nature, the id, privatized objects), thus taking as a primary value the denial of women's subjectivity and personhood. On this reading, then, and within this language of symbolization, inequality and dominance serve as a basis for liberal voluntarist obligation. And this suggests important things about its theoretical, ontological, and epistemological grounding and characterization.

If the conception of freedom as negative is premised on the struggle for recognition, particularly on the ability to be recognized without reciprocation—if nonrecognition is (as it is for the Oedipal boy and Hegel's master) a form of power and violence—freedom, too, must be at least in part an expression of that same power and violence. Within a negative conception of freedom, increases to my freedom will come from a lessening of restriction, such as fewer laws; as positive libertarians will point out, however, this may produce less freedom for those not as well off. Thus, freedom becomes zero-sum, a competitive relationship among beings, some of whom seek to win out over others.

Other concepts based on the premises of that freedom will then, likewise, be expressive of power and violence. Thus, "equality," in referring to abstract opportunity and rights, sets the stage for competition and dominance. Opportunity is equal until someone wins the contest, and even then only for those who start off equal in the relevant respects. "Rights" embodies the concept of claims against others, again suggesting competition and dominance; a right further provides a boundary line between various individuals' needs, desires, and wants and hence serves to divide individuals. In the concept of "equal rights," we must respect each other not because we are connected but because of rights, which highlights the line of demarcation between us. Justice Holmes's witticism, "My right to swing my fist ends where the other man's nose begins" is illuminating as much for its articulation of separate and discrete individual spheres of action as it is for its image of violence; the potential for violence lies at the precise point where discrete individuals have contact.

Obligation, as self-assumed, is a particularly significant manifestation of this conception of freedom because, far more than equality, justice, or rights, obligation centrally involves connection, relationship, and bondedness. Yet a completely voluntarist notion of obligation depends on a conception of people as inherently separate and fragmented, and this resonates strongly with the model of male development within mother-only child rearing. The key principle of consent theory is that one has control over one's bonds or connections to others because one creates those bonds. If I do not wish to be obligated to X, I can simply choose not to incur an obligation to X, I simply need not create it. Creation is a form of power in the sense of control and mastery. The act of consent preserves my right to autonomy as self-determination; it thus asserts my separateness and self-control even as I give up some of that control by creating an obligation. By creating bonds through an act of "free will," I maintain control over myself, self-determination, and freedom.

Obligation within the social contract can be seen importantly as a relationship of exchange. Citizens exchange or trade absolute freedom for security, according to Hobbes; for effective and economic freedom, according to Locke; for the moral freedom of self-mastery, according to Rousseau. Humans give up some liberty to the government and agree to obey it; and in return they receive the goods of a "well-ordered society" (to borrow from Rawls).

But underneath the veneer of this exchange relationship is a relationship of power and domination. Within the logic of social contract and consent theory, one potentially puts oneself in another's power when one places oneself under an obligation by giving up part of one's freedom—one's essence—for something else. In this kind of obligation relation, the obligated person must recognize the obliger in performing the obligation, while the obliger need only accept whatever deed is performed in fulfillment and need not recognize the actor. The dangers inherent in this kind of relationship, this formalized connection, are what require the centrality of voluntarism as the legitimator of such a relationship. If obligation is viewed as a power relationship, being placed in such a relationship without active control over one's placement would seem to make such a relationship doubly coercive.

This is certainly most true for Hobbes, but it is also true for more "social" conceptions of consent theory, like Locke's. The purpose of promising as an institution is to ensure that people will keep their word; and the purpose of the social contract is to enforce such contracts with the threat of sanction. Voluntarism would seem to save the individual from nonreciprocated recognition—from connection itself—by giving the individual the power of control over that relationship. Furthermore, since it is an exchange relationship, each self recognizes the other only to the extent that it chooses, that is, to the extent that it is in its interest. Ostensibly, this would seem to pose a solution to the "master's dilemma"; for both parties, by expressing their interests, control the degree and form of recognition of the other. Consent thus seems to save us from authoritarian coercion.

But in reality it merely masks it. The seeming reciprocity of exchange is belied by the strict theoretical adherence to consent and voluntarism in face of the fact that such consent is nonexistent for all but a select few. In most of the social contract theories, political obligation is not in fact self-assumed in the full sense by very many people, as critics since Hume have pointed out. Certainly Hobbes's approach to consent, as a "rational fiat," leaves very little to actual human choice. Similarly, Locke's myth of tacit consent served to tell everyone that they had in fact consented when they had not even realized it was an option open to them. Further, those who had not actively (if at all) consented were nonetheless considered obligated to obey the law and government. It is rather widely accepted among modern theorists of obligation (see Flathman 1972; Pateman 1985; Simmons 1979; Tussman 1960) that the conditions for true consent are often absent even today. A large number of people are often not given the opportunity to consent, or they "consent" by performing acts about which they have little choice. Even acts of dissent are interpreted as acts of consent, and unfair bargaining positions belie the freedom implicit in free choice.[4] "Tacit consenters" (usually the nonlanded workers, the poor, not to mention women and people of color) were—and many would say still are—subject to the political decisions of the "express consenters" (the landed and wealthy white men who vote and hold political office). Under a liberal voluntarist conception of obligation, then, the class of "masters" is recognized in every sense (political, social, economic), while the "slaves" are not only denied political voice but are told that their imposed silence is voluntary expression and that they are obligated thereby.

Thus, we are led to the suggestion that voluntarist theories of obligation can be read, at least in part, as theories of power, with power conceived as domination. The "freedom and equality" that supposedly founded all voluntarist theories of obligation are merely a patina that obscures the degree to which the fundamental assumptions guiding our definitions of freedom and equality actually apply to a particular group of people and are at least potentially oppressive to others.

The Feminist Standpoint and Feminist Obligation

Why, however, is this a specifically feminist analysis? Women are not the only victims of this conception of obligation; and to that degree there is also a class and race dimension to the problem as well, which will not be analyzed here. Furthermore, it presumes that men will not also use other men of the same race and class in seeking to fulfill the need for dominance.

That the effect is not gender-exclusive, however, does not mean that gender is not an important cause. While object relations theory suggests that the problem of recognition is particularly keen for males, I have also argued that it reflects the experience of the powerful. Since men have power vis-à-vis women, it is to that degree a masculine problem, as well as a white problem and a class, or "bourgeois," problem.

However, the gender dimensions of this problem are particularly powerful because the double-edged exclusiveness of consent theory has special significance for women. The contingent gender bias of voluntarist obligation has portrayed women as incapable of expressing consent and thus of creating their obligations. But even when women's capacity for consent is granted, the structure of voluntarist obligation in fact excludes women by denying the validity of women's experience. Women have been bound historically to an entire series of other obligations—child care most obviously—to which consent is not only often unavailable but often of questionable relevance. Are these "obligations" entirely invalid? Or do they suggest a need to redefine the concept of obligation itself?

The answer is probably a bit of both. Yes, women have been denied opportunities to choose and create their own lives; and these opportunities need to be provided and restored, in part through a "fully consistent" consent theory. Indeed, Enlightenment principles of freedom and equality can be seen as important sources of modern feminism. Many women have left their families, children included; and it can be seen as a form of sexism for us to be horrified by this. But many more women have attempted to carve out choices for themselves without severing relations and responsibilities, for example, by taking their children when they leave a marriage. Many women would balk at the prospect of abandoning their "obligations." One cannot merely blame "false consciousness" or socialization for this. As Gilligan (1982) has argued, "the different voice" engages in moral reasoning from a perspective of relationship and connection, not rights and rules. Such a perspective attunes the reasoner to the needs of others. If a woman takes her children when she leaves her husband, it may well be out of fear for their safety, even though taking them may pose a greater risk to her own safety. Or women might recognize that just as they have been denied choices, so have their children. Should they be the sacrifical lambs to women's realization of their oppression? To say that the women described here have engaged in the process of assessing their options, and have in fact made choices and thus consented, however (even if true consent to their role as mother was not originally given), is disingenuous; for it denies the context within which women are already embedded, a context that places restraints on choice analogous to Locke's tacit consent argument. Rather, the concept of responsibility is the central issue: specifically, responsibility in the positive sense of "responsiveness" (Gilligan 1982).

Obligation needs to be reformulated to account for these values and perspectives, for the very human experience of choicelessness, and for the fact—so adamantly denied by consent and social contract theory—that choices exist in contexts. Indeed, choices are so deeply embedded in contexts of relationship, emotion, value, and taught beliefs—all of which are *social* phenomena, deriving from relations with others and not from a purified or natural self—as to make consent theorists' construal of choice difficult to fit into a realistic picture of human life.

But how would obligation be reformulated along these feminist lines? If the philosophical priority of freedom in part reflects an orientation toward conceiving the self as separate and if women define themselves in relation to others, it follows that a feminist ontology and epistemology would operate from the philosophical priority of obligation. Object relations would then provide us with the basis for suggesting how individual experience at least impinges on institutions like the state as well as on the political philosophy that defines or justifies it.

Accordingly, then, a feminist conceptualization of obligation would operate from a different framework, namely, the givenness of connection and responsibility. The epistemological perspective of enmeshment in a web of relationships leads to the moral conclusion that freedom—the central element in social contract theory—is, while certainly important to the concept of a person, not necessarily the primary, or central, element. A prior and more central concept would be responsibility in the sense of response, or even obligation itself; that is, from a "feminist standpoint," perhaps obligation needs to be taken as given.

To make sense of this suggestion requires a new framework of inquiry. Working from an assumption of separateness and freedom, consent theorists seek to understand how separate individuals can develop and sustain connections and still be separate, how they can engage in relationships and still remain free. Thus, the central approach involves determining how obligations "arise," how they come into being. But if obligation is given, it does not really make sense to ask how it can arise. Obligation is the standard against which other things, such as the freedom to act as one wishes, are measured. In liberal obligation, the assumption of freedom demands an explanation of any curtailment of that freedom, such as obligations impose. In this feminist conception, such a demand violates the imperative of responsibility and care, and it is the assumption of obligation that demands an explanation of *nonfulfillment.* This different orientation requires inquiry into the contextual conditions surrounding an obligation and obligated person so as to understand the content of an obligation, rather than the process, as well as to understand possible justifications for nonfulfillment. Such an inquiry would examine the concrete particularities of a situation to articulate the conflicting pressures that lead to a particular action as the fulfillment of the obligation, or that provide a possible reason for not fulfilling it. Put rather simplistically, this approach can be seen as more interested in what particular obligations consist of than in how they come to be.

Thus, a feminist method profoundly alters the very terms of the discourse. Beginning with the self as separate, liberal voluntarist obligation seeks to find areas and modes of connection that are safe—that can provide for needs without risking the loss of self. This feminist model, beginning with connection, tries to determine how to carve out a space for the self without violating care. While freedom is certainly achievable in the context of human relationships, it must *be achieved*, it is not a given. Freedom is an entity that must be created, as an individual carves out space for him- or herself. And since freedom is created by a stepping away from, or out of, obligations, freedom must also be justified. Relations cannot be severed by a "mere" desire or act of will. There must be, to borrow from Flathman (1972), "good reasons" for the desire *not* to fulfill obligations.

This does not mean such justification is not possible or even likely for a wide variety of cases. In a century that has seen Hitler, Stalin, and Jim Jones, the concept of a given obligation may make some uneasy. A second source of unease comes specifically from women's experience. Nonconsensual obligations have been imposed on women all their lives. Isn't it time the yoke was shaken off? Should not women be able to choose their obligations, just as men have?

I share these concerns: within the liberal framework, such a construal does not make sense and is indeed nightmarish. But at the same time, these objections miss the depth of the reformulation suggested here. The purpose of a feminist approach to obligation is not to bind us more tightly to the state or to relations; nor am I suggesting that we model political obligations on the relationship between mother and child. Rather, object relations points out that women's experience, which is systematically eliminated from such public ideologies as political theory, can tell us important things about *human* life. It enables us to see the reality that women's lives reveal, that is, that we—men and women alike—are often in fact nonconsensually bound more tightly than our public discourse admits.

Consent theory tells only part of the story of human experience; it presents, therefore, a biased and distorted picture of obligation. The ideology of consent allows us to believe that all obligations are created. We can thus deny the obligatory force of any relations that we do not create or do not wish to maintain. This *we*, however, is largely masculine; for this belief exists, operates, and flourishes in the public realm. In the private realm, women's realm, obligations are often not at all consensual.

At the same time, one cannot merely add women's experience to the dominant discourse because the two utilize different ontological and epistemological frameworks. The problem of women's obligation exists within the context of social institutions and thought that create two different sets of values for men and women—men are naturally free, women are naturally obligated. Within this context, of course, feminists do not wish to maintain the givenness of obligation for women, for that would perpetuate their inferiority. Rather the context must be changed.

But to reverse this order simplistically—to obligate men nonconsensually, as women have been obligated, to "drag them down" to women's oppression—is certainly not a goal. Rather, the point is to call attention to the fact that men already *are* nonconsensually obligated in many ways and that these obligations are appropriate to human relations but that our public ideology will not allow us to recognize this fact. Such a refusal obliterates the hope of a human theory of obligation that recognizes choice *and* givenness. Similarly, women have the capacity to create many of their obligations but are effectively denied the opportunity to do so. This denial, similarly, obliterates the hope of a human theory of obligation.

Conclusion

Thus, one must realize that the claim that obligation should be considered as given is an epistemological and methodological claim. It strikes to the heart of how we conduct theoretical inquiry, what sort of questions we ask, and how we formulate them. Certainly, I have provided only a sketch of the positive side of my task, the beginning of an answer to the question of how to reformulate obligation. But the critical dimensions of this feminist theoretical approach are clearly powerful. Through application of the standpoint epistemology method, I have shown how the gender-related experiences that object relations theory articulates can be used to gain a deeper understanding of the epistemological grounding of obligation and of the structural gender bias that voluntarist theories embody. A similar method could be applied to other concepts, like freedom and rights. By asking this very different set of questions, a feminist approach to obligation redefines the issues and problems that political theory needs to addresss. In doing so, it points new directions for theory to take.

Notes

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1. While I will make several remarks on the concept of duty throughout the essay, I will not undertake a detailed discussion of the concept. There are aspects of duty that would prove useful to my analysis, but that is not the project I am engaged in here. My point is that liberal theory places consent at the heart of politics by making it the model for all human relationships. In circular fashion, this is precisely why duty has not been as central to liberal theory, as obligation, justice, rights, and freedom have been. Fishkin's (1979) notion of "general obligation" is also potentially useful to my theory, but I disagree with his claim that nonvoluntary obligation is compatible with liberalism. See Hirschmann n.d. for a fuller discussion of Fishkin.

2. Gilligan's thesis has bred considerable controversy, for which I refer the reader to other discussions (Kittay and Meyers 1987; Tronto 1987).

3. The obvious, if ambivalent, exception is women's relation to the family. It would bring us into an entirely different discussion, but the family plays a curious and varied role in these theories, as relations between men and women are alternately cast as natural and contractual. But even when the latter prevails, women do not have the same powers and rights as men.

4. I refer here not only to using the roads, which fairly clearly does not correspond to any meaningful interpretation of consent, but also to such liberal democratic practices as voting and civil disobedience as well. See Hirschmann n.d., Pateman 1985, and Piven and Cloward 1988 for relevant discussions.

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BIPOLARITY, MULTIPOLARITY, AND FREE TRADE

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Recent literature typically attributes the relative scarcity of open international markets to the prisoner's dilemma structure of state preferences with respect to trade. I argue that the prisoner's dilemma representation does not reflect the most critical aspect of free trade agreements in an anarchic international system: security externalities. Explicit consideration of these effects suggests that a bipolar international political system has an advantage relative to its multipolar counterpart with respect to the opening of markets among states. Less credible exit threats and stronger incentives to engage in altruism within its alliances explain the advantage of a two-power system.

The real income gains that motivate free trade are also the source of the security externalities that impede it. Their ability to internalize these effects makes military alliances the natural basis of agreements to open international markets. The evolutionary prospects of alliances vary, however: ones that are the products of bipolar systems are more likely to evolve into free trade coalitions than are their multipolar counterparts. I argue that less credible exit threats and stronger incentives to engage in alliance altruism explain the advantage of a two-power system.

Several important limits of the argument should be made clear immediately. I argue at the systemic level: I attempt to isolate the political and economic incentives for free trade that occur at the level of the international system. I do not, therefore, consider the impact of unit-level factors on the pursuit of these incentives. For example, neither the role of special interest groups nor the organization of domestic exchange via hierarchies instead of markets is considered (Doyle 1986); Frieden 1988; Ruggie 1982). The conditions I assume are those of standard international trade theory (Ethier 1983), and illustrative rather than systematic empirical referents are used. These restrictions are appropriate given my purpose: to demonstrate analytically the advantage of a bipolar international political system with respect to free trade.

Open Markets, Prisoner's Dilemmas, and State Power

The earliest variant of hegemonic stability theory bases its representation of international trade as a Prisoner's Dilemma (PD) on the public good literature (Kindleberger 1973). It claims that a stable system of international free trade involves the supply of a public good. Such goods are joint in supply and nonexcludable, that is, (1) any individual's consumption of these goods does not preclude their consumption by others; and (2) no individual can be excluded or prevented from consuming such goods, whether he or she has contributed to their production or not. As a result, the preferences of each member of a large, or of any short-lived, group facing a public good problem conform to a PD. The corresponding payoff

Table 1. The Prisoner's Dilemma

	Column	
Row	Cooperate	Defect
Cooperate	2, 2	4, 1
Defect	1, 4	3, 3

Note: Payoffs are ranked from 1 (best) to 4 (worst); row's payoffs are listed first.

matrix is illustrated in Table 1. Given these payoffs, the dominant strategy of each player is to defect, or refuse to contribute to, the supply of the public good.

This leads to an equilibrium outcome of DD that is Pareto-inferior (i.e., another equilibrium exists in which at least one individual would be better off and no one would be worse off than at the existing equilibrium). In this equilibrium no one contributes, no public good is produced, and another—albeit unstable—Pareto-superior outcome exists (CC). To achieve a stable free trade equilibrium, hegemonic stability theory asserts, a hegemon or dominant state must exist (Kindleberger 1973, 305). This is the equivalent of Mancur Olson's privileged group; that is, a group "such that each of its members, or at least some one of them, has an incentive to see that the collective good is provided, even if he [alone] has to bear the full burden of providing it" (Olson 1971, 50). While they accept the PD representation, critics assert that the logic of public good analysis itself indicts hegemonic theory: because small, or *k*, groups can also provide public goods, either a hegemon *or* a small group of states can stabilize a free trade system (Keohane 1984; Snidal 1985; Yarbrough and Yarbrough 1987).

The PD representation can be derived as easily from standard international trade theory (Conybeare 1984). The latter points out that any state large enough to influence its terms of trade—the relative price of its exports—maximizes its real income by imposing an "optimum" tariff, that is, a tariff set at a level that maximizes the net gain that accrues from the improved terms and reduced volume of trade.[1] If optimum tariffs are used by all states, however, an individually and collectively suboptimal outcome results: the volume of trade is reduced, but the terms of trade do not change.[2] The mutually preferred outcome of free trade is difficult to achieve because it is not a stable or Nash equilibrium of the one-shot game: each state is better off defecting to an optimum tariff if others do not. Thus, standard trade theory also characterizes trade among large states as a PD game.

Both the public good variant of hegemonic stability theory and standard international trade theory, however, analyze economic exchange in a political vacuum: they focus exclusively on the real income gains that accrue to a state that opens its borders to trade. Yet national power is engaged in free trade agreements because such agreements produce security externalities: the removal of trade barriers can affect not only the real income but also the security of the states concerned.[3]

The security externalities of trade arise from its inevitable jointness in production: the source of gains from trade is the increased efficiency with which domestic resources can be employed, and this increase in efficiency itself frees economic resources for military uses (Root 1984). Thus, trade increases the potential military power of any country that engages in it (Albert O. Hirschman, cited in Baldwin 1985, 211). In doing so, it can disrupt the preexisting balance of power among the contracting states (McKeown 1982, 225).[4]

Because it is locked into the insecurity that an anarchic international system almost invariably produces,[5] a great power is less likely to be concerned about the absolute income than about the relative power effects of trade. The structure of international politics makes all states aware that each seeks to exploit the wealth of others to enhance its own power. That trade is a means to this end is also common knowledge: as David Baldwin observes, private international trade "is by far the most [cost-]effective . . . way for one country to acquire the goods or services of another" (Baldwin 1985, 116). As a result, states do not calculate their payoffs from trade only in real income terms: relative power effects are likely to affect their calculations strongly.

The interwar period provides a compelling example of the power of the security dilemma to contribute to a free trade deadlock. The acrimony of the international economic diplomacy that preceded the 1933 World Economic Conference suggested that the conference would not secure a free trade truce. In the early interwar years, the payment of German reparations was the subject of repeated negotiations. Ostensibly a technical issue whose resolution depended upon expert testimony regarding Germany's ability to pay, the payment of reparations was in reality a highly political issue: the integrity of the entire Versailles settlement was at stake (Trachtenberg 1980, 122).

In negotiations that extended from the Paris Peace Conference of 1919 to the Lausanne Conference of 1932, the issue of reparations exposed the political roots of international economic diplomacy. Determined to minimize its involvement in continental politics, Britain insisted that the stabilization of Europe depended on the restoration of financial stability and economic prosperity to Germany. It opposed the provisions of the Versailles settlement that it believed either inhibited the economic recovery of, or contributed to financial instability in, Germany (Schuker 1976, 384). As a result, Britain was sympathetic to German demands for a reduction in reparations.

France, however, was reluctant to endorse any revision of the Versailles settlement favorable to Germany. Unable to persuade either Britain or the United States to guarantee its security, France regarded the retention of the superior position accorded to it in the treaty as its only alternative. Thus, "the sanctity of treaties and the strict enforcement of the *status quo*" became the first principles of French foreign policy (Wolfers 1940, 19). Because it viewed any treaty revision as setting a dangerous precedent, France engaged in a determined effort to compel Germany to comply with its reparations obligations.

Repeated negotiations on German payments did little to reconcile the underlying conflicts of interests among the great powers. Neither Britain nor the United States came to appreciate the French sense of acute vulnerability; France never accepted the Anglo-American vision of an economically prosperous and politically satiated Germany; and Germany never deviated from its strategy of destroying the Versailles settlement.

Thus, the economic diplomacy that preceded the 1933 World Economic Conference left a very unpromising legacy: a pattern of intense conflict over fundamental security issues among the great powers. It is no surprise that conflict rather than cooperation marked the 1933 Conference as well, although factors other than historical precedent contributed to its conspicuous lack of success (Eichengreen 1988; Oye 1985).

The interwar example obviously does not imply that political conflicts necessarily obstruct the realization of gains from trade. In theory, these conflicts need not paralyze states. They can instead be resolved through the use of a two-step process in which states initially dismantle trade barriers between them; subsequently, each adjusts its defense strategy to accommodate any changes in the balance of power that occur as trade barriers fall. The danger that a window of vulnerability may open between stages, however, is a powerful deterrent to this approach: the historical record suggests that such windows can open surprisingly quickly (Trachtenberg 1988).

An obvious alternative is to negotiate trade and security agreements simultaneously. Suppose, for example, that the Soviet Union and the United States wanted to trade freely with each other but were concerned that the balance of power between them might shift as a result. In theory, the two countries could resolve their fears by simultaneously negotiating an arms control accord. In effect, this solution compresses the sequential process just outlined into a single stage.

The costs of implementing this solution may be prohibitive, however. As the opening shots in the Uruguay Round make clear, the negotiation of a trade agreement alone, even among allies, is very complex; its success is uncertain at best. If the merger of negotiations itself does not affect the incentives to conclude either accord, an explicit linkage of trade and security issues between potential or actual adversary states raises the costs and lowers the probability of success by several orders of magnitude. Thus, assuming that gains from trade do not vary widely across potential negotiating partners, it is cheaper for states to negotiate with their allies on trade alone than to link trade and security issues in a comprehensive negotiation involving possible or actual adversaries.

The historical record demonstrates that more powerful incentives than the gains from trade have been necessary to control arms races. Analyses of the nineteenth and twentieth centuries conclude that the majority of arms races that have ended peacefully have done so because of changes in the macropolitical order—specifically, the emergence of a threat by a third state to both participants in the race (Downs, Rocke, and Siverson 1985). The rise of German power, for example, ultimately ended the British-French naval race of the midnineteenth-to-early-twentieth centuries (Downs, Rocke, and Siverson 1985, 119–20).

Thus, although states can in theory successfully confront the problems inherent in the security externalities of free trade agreements, analysis and evidence suggest that such successes will be rare events in world politics. It is, as a result, not surprising that students of trade regimes either explicitly or implicitly link their analyses to the postwar Western alliance (cf. Haggard and Simmons 1987, 134; Keohane 1984; Yarbrough and Yarbrough 1986). Nor is it surprising that some observers argue that tariff cuts are more likely between allies than between states belonging to different military coalitions (McKeown 1982, 225).

Not all military alliances are equally likely to evolve into free trade coalitions, however. I shall argue that alliances produced by bipolar systems are more likely, and ones formed within multipolar systems are less likely, to so evolve.

Great Powers, Alliances, and Free Trade

International systems are distinguished here in terms of individual states rather than in terms of alliances (Rapkin 1988). Thus, the definition of multipolar and bipolar systems used here follows that of Synder and Diesing: the structure of an international system

> is defined by the *number* of major actors in the system and the *distribution of military power* and potential among them. In a multipolar system there are several (more than two) 'Great Powers' whose military power is roughly equal, and whose rivalry and cooperation dominate politics in the system. . . . A bipolar system is one with only two Great Powers and a number of smaller states. (Snyder and Diesing 1977, 419–20)

If the security externalities of any free trade accord render military alliances the natural basis of such agreements, the PD incentive structure confronting states that seek security in a multipolar system makes alliance formation likely: the fragmentation of the system into rival alliances is much more likely than a tacit or explicit general assignment to abstain from such alliances (Snyder 1984, 462). That a similar dynamic has occurred in the postwar bipolar system is clear.

In principle, neither the alliances of a multipolar, nor those of a bipolar, system should enjoy an advantage with respect to the evolution of a corresponding free trade coalition: alliances in both systems render the security externalities that accrue to a state's trading partners positive rather than negative. Yet a bipolar system *is* advantaged relative to its multipolar counterpart on two dimensions relevant to the conclusion of an agreement to open intraalliance markets: (1) exit and (2) incentives of the great power members to forgo the use of their market power to exploit the smaller members of the alliance.

The Costs of Exit

The threat that any member can exit or abandon an existing alliance to join an alternative one reduces the likelihood that allies will open their markets to each other: exit transforms the external security economies that would otherwise facilitate the conclusion of an accord into *dis*economies. As exit opportunities recreate balance-of-power fears and also shorten the "shadow of the future," prospects for free trade even within international alliances seem dim.

As Snyder observes in his discussion of the alliance security dilemma, however, the risk of exit varies widely across international systems: the risk is higher in multipolar than in bipolar international political systems (Snyder 1984). Snyder's hypothesis has been confirmed empirically: studies of the nineteenth and twentieth centuries demonstrate that exit is indeed more likely to occur in *n*-power than in two-power systems (Duncan and Siverson 1982).

The number of great powers is obviously not the only source of variations in alliance durability. In the postwar period, for example, the stability of the Western alliance is the product not only of shared strategic interests but also of the democratic polities and mixed economies common to its members; analogously, the Soviet Union's determination to maintain ideologically compatible states along its western frontier tightens bonds among members of the Warsaw Pact. But idiosyncratic factors alone do not explain the postwar alliance configuration: analytic arguments also predict that bipolar systems generally will produce more stable coalitions than will their multipolar counterparts.

First, as Snyder argues, bipolar coalitions are the product largely of systemic structure. Alliances in a multipolar system, however, are not structurally determined but are the result instead of "choice among several options" (Snyder 1984, 415). Typically, serious conflicts of interest among the great powers of any given system result from the anarchic character of international politics. As a consequence, the substitutability of great power allies is likely to decline with their number: the probability of finding another large power as compatible as the existing ally varies directly with the number of great powers that exist. Thus, the great powers of a two-power system are less likely than are those of an *n*-power world to be close substitutes for each other.

This can be represented spatially, where the distance between any two states is a measure of the conflict of interest between them. A bipolar system embedded in an anarchic world is likely to distribute its constituent great powers 180 degrees from each other. Thus, a very sharp distinction is likely to exist between the two great powers of any bipolar system. As a consequence, each is likely to appeal to a very different set of potential allies, and the allies themselves will be highly constrained both with respect to their initial choice of an alliance partner and with respect to their ability to shift alliances thereafter.

The addition of one or more states to the system automatically increases the range of choice available to allies. Because this change necessarily reduces the distance between states relative to a bipolar system, it also increases the range of alliance choices and the opportunities for alliance shifts relative to a two-power world. As a result, coalitions in an *n*-power system, unlike alliances in a bipolar world, tend to be "unstable and vulnerable to policy disagreement" (Snyder 1984, 415).

A second reason that different exit risks exist is that the distribution of information about potential allies is likely to vary across systems. The industrial organization literature distinguishes between *search* goods and *experience* goods. Applied to international alliances, this distinction illustrates the impact of variations in information on coalitional stability. In economics, consumers have complete information about the characteristics of search goods before purchase (a dress, for example); experience goods are products whose characteristics become fully known to consumers only after purchase (a restaurant's quality, for example) (Tirole 1988, 106).

Because the amount of reliable information about potential great power allies is likely to be inversely related to their number, alliances are more likely in a bipolar system to resemble search goods and in a multipolar system to resemble experience goods. Thus, the passage of time leads to greater learning about alliance partners in a multipolar world than it does in a bipolar world. To the extent that alliance shifts are a function of the reduction of uncertainty over time, they are more likely to occur in an *n*-power world than in a two-power world.

Finally, exit risks are likely to be lower in a two-power system because realignment is impossible for either great power. By default, alliance stability is also the exclusive responsibility of each: neither of the two great powers in such a system can expect any other state to prevent the defection of an ally from within its bloc (Snyder 1984). In a multipolar system, the interest in preserving alliance stability and the incentive to do so can be distributed across more than one great power. As a consequence, each may seek to transfer the burden of maintaining the alliance to the other.

Thus, a strong analytic argument, as well as empirical data, supports the hypothesis that the risk of exit varies across international systems. This variation, in turn, induces variation in the discount rates of alliance members. Allies in a multipolar system will tend to discount the future benefits accruing from open markets among them more heavily than will their bipolar counterparts. The relatively lower value they place on the future relative to the present reflects their recognition of the "likelihood that the future will not come" (Levi 1988, 13). The discount factors of allies in a bipolar system, in contrast, are not subject to the same downward bias: the greater stability of bipolar coalitions allows the value of future to approximate present benefits more closely.

An analysis of relative exit risks suggests that the security externalities of any free trade agreement are more likely to remain internalized within the alliances of bipolar systems than they are within alliances of multipolar ones. As a result, both the economic and political benefits of intraalliance free trade are likely to be discounted less heavily in a two-power than in an *n*-power world. Thus, the dimension of exit advantages a bipolar system with respect to the opening of markets between or among states.

Intraalliance Exploitation: Incentives and Constraints

The presence of both great and small powers within an alliance raises the issue of exploitation in sharp relief. Assuming that "great" and "small" are good proxies for market power,[6] the tariff game between allies is an asymmetric one: a large state can, and a small state cannot, use tariffs to improve its terms of trade. In this game, the dominant strategy of the large state is to defect to an optimum tariff; the dominant strategy of the small state is to cooperate or adhere to free trade (Conybeare 1987, 35). Table 2 displays the payoffs to the small (row) and to the large (column) power. The logical outcome of the game is CD: the small state abjures and the large state erects tariff barriers. Under what conditions, then, will free trade occur within an alliance? What will persuade the intraalliance hegemon to forgo an optimum tariff?

Table 2. The Tariff Game between Allies

	Column	
Row	Cooperate	Defect
Cooperate	1, 3	3, 1
Defect	2, 4	4, 2

Note: Payoffs are ranked from 1 (best) to 4 (worst); row's payoffs are listed first.

Incentives to exploit. Critics of hegemonic stability theory argue that there are strong incentives for *any* hegemon to use an optimum tariff (Conybeare 1984, 1987). On economic grounds alone, however, a *nonmyopic,* rational hegemon may reject such a tariff for the same reason that a domestic monopolist sets prices below their short-run maximizing levels: to deter competition.[7] By "limit" pricing, the monopolist seeks to convince potential entrants that its costs of production are lower than they are in reality (Milgrom and Roberts 1982). The incumbent firm's ability to sacrifice short-run gains in order to earn higher, long-run returns depends on the existence of costs to entry as well as on asymmetric information about its costs of production.[8]

A rational, nonmyopic hegemon may set its tariff at less than the short-run optimum level under analogous conditions: if, for example, it has some private information about the elasticity of global demand-and-supply curves and if small countries organizing to exert countervailing power in world markets incur some costs in doing so.[9] That a hegemon might indeed have private information about global markets follows logically from its incentives to become informed about them; a small country, in contrast, has little incentive to acquire such information, because it assumes that it cannot affect its terms of trade. That significant transaction costs are incurred in the process of forming customs unions follows from the distributional effects (both within and across the custom union's potential members) evoked by the setting of uniform external trade barriers (McMillan 1986, 67).

The trading practices of both Britain and the United States suggest that the analogy to limit pricing is of more than just analytic interest. Midnineteenth-century Britain, according to one observer, maintained its tariffs at less than optimum levels in order to fix its "monopoly of manufactures on the rest of the world for a few more decades than its natural term" (William Cunningham, cited in McCloskey 1980, 304). The logic of limit pricing apparently became clear to the United States when it attempted, in the 1930 Smoot-Hawley tariff, to turn the terms of trade in its favor. This effort, among other factors, provoked the construction of trading blocs abroad and apparently induced the United States to try to lower global barriers to trade after the war.

In short, because an attempt to exploit its power in the short run may undermine that power over time, a nonmyopic, rational hegemon may reject an optimum tariff. Although the limit pricing argument does not support an inference that free trade—the international analogue of competitive prices at the domestic level—will prevail, it does suggest that unilateral restrictions on the use of an optimum tariff can be in the strictly economic self-interest of a far-sighted, clear-thinking hegemon—whether within or outside an alliance.

Because the limit pricing analogue suggests that it may be cheaper to forgo an optimum tariff than standard trade theory implies, it also suggests that the incidence of decisions to do so for political reasons and the incidence of intraalliance altruism may be higher than would otherwise be expected. Experimental results indicate, for example, that the frequency of altruistic behavior is inversely related to its cost (Frohlich et al. 1984, 23). Thus, in its own self-interest, a great power may choose to forgo the use of an optimum tariff against its smaller allies.

Constraints on exploitation. A great power's ability to exercise its monopoly power depends not only on its economic incentives to do so but also on the constraints others can impose on it. In a variety of contexts (Levi 1983, 1988; Popkin 1986), constraints on the ability of the more-powerful to exploit the less-powerful are attributed to the dependence of the "rulers" on the welfare of those they rule and to the existence of competition or of "rivals to rule" (North 1981).

Analogous constraints limit the exploitation of the small by the large within alliances. The dependence of each great power on the welfare of its allies gives each an incentive to define its self-interest altruistically, where altruism means simply that its "utility function depends positively on the well-being" of its allies (Becker 1981, 173). Thus, the great power derives less utility from intraalliance income transfers than from other international income transfers (see Conybeare 1987, 88).

Indeed, each great power in an alliance may actually be better off if it renounces the use of trade barriers: the marginal utility to it of an increase in the income of an ally may be greater than the marginal utility to it of a smaller increase in its own income. Thus, all members of an alliance may be better off if the great power or powers pursue free trade, just as the utility of two individuals may be enhanced by a transfer of income from one to the other (Boadway and Wildasin 1984, 116). That this is plausible in the context of intraalliance trade is suggested by the efficiency losses associated with the use of tariffs (Caves and Jones 1973, 244) and by the large welfare improvements that typically accrue to small states as a consequence of trade.

Intraalliance altruism is more likely to be supplied within the coalitions of bipolar systems than within the coalitions of multipolar ones. Both the threat of exit and the hope that another great power member of the alliance will act altruistically make it improbable that interdependent utility functions will motivate "effective" altruism (i.e., an actual change in behavior [Becker 1981, 173]) in a multipolar world. Exit creates the risk that income forgone will redound to the advantage of another alliance. Vulnerability to free riding threatens the expression of altruism, because any great power is better off if another acts to advance the collective welfare of the alliance than if it does so itself; thus, as is true of coalition maintenance, each state may pursue its national rather than alliance interests (Collard 1978, 13).

The competitive constraint on exploitation, however, might appear to be stronger in a multipolar system than in a bipolar one: the relative scarcity of exit options in the latter would seem to create ample opportunities for exploitation. As McKeown points out in a more general context, for example, Britain could impose "highly favorable commercial treaties with [sic] the South American states"; it was less successful with Spain and Portugal, who could seek French support if British demands became excessive (McKeown 1982, 214). The rivalry-to-rule constraint, though, does not arise exclusively from allies' opportunity costs of exit. It inheres as well in the competition between or among alliances as a whole that an anarchic international structure typically produces.

Although it is theoretically possible for the great powers of any system to collude in the exploitation of their allies, it is more likely that the noncollusive outcome will emerge (Snyder 1984, 462). In its attempt to balance the power of the opposing bloc, each great power is more likely to pursue the individually rational but collectively suboptimal strategy of investing in the welfare of its allies. The pattern of investment is likely to vary across systems, however.

Any great power member of a coalition in a multipolar world has strong incentives to discriminate among its allies in terms of its investment in them, as its allies presumably confront divergent opportunity costs of exit. In contrast, the incentive to discriminate in a bipolar world is much weaker, as allies are much more uniformly and securely locked into coalitions. Thus, in the trade arena, for example, investments in allies in a multipolar system are likely to be expressed in the form of discriminatory trade preferences; in a bipolar system, free trade is the more likely outcome.[10]

Conclusion

The security externalities of agreements to open borders to trade, I argue, imply that these agreements are more likely to occur within than between military alliances. More importantly, the evolution of alliances into free trade coalitions is more probable in a bipolar than in a multipolar system. Crucial to the advantage of a two-power system are the lower threat of exit and the stronger incentives for intra-alliance altruism that it confronts.

This analysis sheds some new light on several debates about the causes of free and controlled trade during the nineteenth and twentieth centuries. It effectively assigns a large role in the opening of postwar Western markets to the transition from a multipolar to a bipolar international security system that occurred simultaneously. It also implicitly attributes the breakdown of trade in the interwar period partly to the existence of a multipolar security dilemma. It provides a good fit to arguments that Britain was neither a political hegemon nor a strong advocate of lowering trade barriers elsewhere during the nineteenth century (McKeown 1983; Russett 1985).

Of course, the argument can also be indicted on several counts. Any attempt to test it empirically will confront many problems familiar to students of international relations: for example, the small number of international systems during the period of modern international trade precludes statistical analysis, and it is difficult to use comparative case studies because of large variations in important variables between or among the cases. In addition, the problem of the single case—that of bipolarity—arises. One way to address the latter problem might be to break apart multipolar systems, since these systems vary in terms of alliance flexibility: ones that have experienced an "integrative spiral" should more closely resemble bipolar systems than ones that have not.

Problems also exist at the analytic level. Vulnerable to challenge are my implicit or explicit assumptions that incentives to exploit, the utility of allies, and the importance of security externalities can be held constant across systems. The neglect of domestic factors is also susceptible to criticism, particularly because trade within the Soviet alliance system is determined by the same hierarchical controls that operate within the Soviet economy.

It is also true, however, that an effort to include all variables that might influence trade in the international system can succeed only at the price of analytic intractability. The merits of the analysis presented here, therefore, must be judged in terms of the importance, persuasiveness, and originality of the two arguments central to it: (1) that the representation of the problem of free trade as a PD game neglects the security externalities that arise in the international system and (2) that a two-power system has an advantage with respect to the opening of markets among states.

Notes

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1. Technically, the size of the country to which the optimum tariff argument applies "is not the size of the country as a whole but rather its share of world trade in markets in which it exports and imports" (Deardorff and Stern 1987, 37).

2. This assumes a symmetrical distribution of market power. If an asymmetry exists, however, one state may be better off even after the cycle has been completed (Johnson 1953).

3. More generally, an external economy "is said to be emitted when an activity undertaken by an individual or firm yields benefits to other individuals or firms in addition to the benefits accruing to the emitting party" (Boadway and Wildasin 1984, 60). External diseconomies inflict injury rather than confer benefits.

4. These two paragraphs are based on Gowa 1989, 323.

5. See Jervis 1978 for discussion.

6. However, see n. 1.

7. This analysis of limit pricing is from Gowa 1989, 312–14.

8. Since potential entrants are aware of the incentives of established firms to engage in limit pricing, the established firm's strategy may not work. See Milgrom and Roberts 1982 for a complete, formal analysis of the limit pricing game under incomplete information.

9. In his most recent work, Conybeare notes that heavy export taxes may induce substitution that in turn, dictates the use of lighter taxes in the interest of maximizing long-run profits. Conybeare dismisses this argument unpersuasively: he claims that "long-term elasticity . . . arguments merely assert that the hegemon is not really a hegemon" (Conybeare 1987, 72).

10. This analysis assumes that the value of allies is constant across multipolar and bipolar systems (cf. Waltz 1979, 168). If, for example, the great powers of a bipolar system together command 60% of the world's power resources, available allies by definition command the remaining 40%. If three powers account for 60%, their potential allies still command the same proportion of power resources as do those in a bipolar system. For an extended discussion, see Snyder and Diesing 1977, chap. 6.

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WORKER INSURGENCY, RADICAL ORGANIZATION, AND NEW DEAL LABOR LEGISLATION

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Debates over the reasons for the passage of class legislation during the New Deal era have been of continuing interest to social scientists. Of special importance has been the problem of explaining the passage of the 1935 National Labor Relations Act (NLRA), often considered the most significant and radical bill of the period. In this article, I examine the influence of worker insurgency and radical organization on the passage and final form of the NLRA. I argue that other analytic approaches fail to take into account the importance of this influence and the degree to which it constrained and structured the responses of key political actors. I conclude that the theories that downplay the importance of worker insurgency and radical organization are both wrong in the particulars and suspect as general theories; this applies especially to the perspective that emphasizes the autonomy of the state from societal forces.

Discussions in the social science literature about the reasons for the passage of class legislation during the New Deal period have become quite contentious recently[1] (Domhoff 1986, 1986–7, 1987; Ferguson 1984; Quadagno 1984, 1985; Skocpol 1980; Skocpol and Amenta 1985). These debates raise important issues of wide interest, including fundamental questions of U.S. politics, the nature of the modern state, and basic problems of social science methodology. Yet they may also be characterized by their neglect of what I will argue is a central issue. Although the 1930s represented a high-water mark for labor insurgency, broad social movements, and radical organization, few of the participants in the debates over the New Deal have considered these factors to be important influences in national politics.

It is not by accident, of course, that discussions of fundamental questions of U.S. politics should focus on New Deal social legislation: the New Deal is often regarded as the beginning of an activist state in the United States, when class-based legislation emerged as a major item on the political agenda; the electoral realignment represented by the New Deal ostensibly enlarged the political arena to include workers, Afro-Americans, and the poor generally; it also was a time of great stress and conflict, when contending forces struggled over the reshaping of policy and politics and hence, when certain aspects of politics and social life were more exposed to view. Class legislation passed during the New Deal period is sometimes described as "radical" (Leuchtenburg 1963, 336), even "revolutionary" (Brandeis 1957, 195, 198). That piece of legislation to which the most extreme adjectives have been applied is undoubtedly the 1935 National Labor Relations Act (NLRA or Wagner Act) referred to as "innovative" (Skocpol 1980, 159), "radical," and "one of the most drastic legislative innovations

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of the decade" (Leuchtenburg 1963, 151). In typical hyperbole, Carl Degler describes it as "perhaps the most revolutionary single measure in American labor history" (Degler 1984, 436–37). Karl Klare, a critical legal theorist, similarly calls it "perhaps the most radical piece of legislation ever enacted by the United States Congress" (1978, 265). Whether or not these accolades are deserved, the NLRA was clearly not a routine piece of legislation. It was one in which labor organizations, corporations, and many other groups had a keen interest and a major stake in influencing its outcome (both passage and final content). Thus, if one wants to examine how groups, classes, parties, state capacities, organizations, and structures influence fundamental issues of public policy and especially whether labor militancy, social movements, and radical organization are important to consider, the passage of the NLRA is a reasonably good place to start. Equally important, it is an important test case that all analysts of the New Deal and theorists of the state believe their approach is best able to explain.

In this article I attempt to document and argue that labor militance and radical organization did have major influence on the passage of the 1935 NLRA. Though this corrective, I would argue, is not unimportant, my real intention and hidden agenda is to open a Pandora's box of key issues for the study of U.S. politics and the study of the modern state. I want to suggest the importance of the past and potential effects of broad social movements in affecting U.S. politics.[2] Thus, I wish to open the door to remedying a general neglect not just for the 1930s but certainly for the 1960s as well, suggesting that the weaknesses and strengths of these movements have done much to shape the contours of U.S. politics and the state. Finally, I will argue that no theory or research agenda for study that ignores these factors can prove to be complete or adequate.

Background

Prior to the 1930s, unions, whatever their legal status (and this varied by state), were de facto illegal. Employers could often threaten, intimidate, and fire their workers, who themselves had little recourse. In the case of strikes, workers could be imprisoned, and their unions could easily be served with injunctions and destroyed. In all too many cases, employers with their private police forces (or public ones that followed their directives) would arrest, beat, and murder militant workers with impunity.[3] While certain of these employer activities were illegal (though rarely punished) many successful weapons for combating unions were quite legal. Two of the main such legal tactics were the yellow-dog contract, a hiring agreement in which a worker pledged never to join a union, and easily obtained court injunctions, making unions responsible for a whole range of nebulous damages (Frankfurter and Green 1930; NLRB 1985, 2382–84).

A steady stream of labor legislation during the 1930s wiped out the legal basis for antiunion employer tactics. The 1932 Norris-LaGuardia Act declared the yellow-dog contract unenforceable and greatly limited the use of injunctions. The 1933 section 7(a) of the National Industrial Recovery Act (NIRA) asserted—though with no enforcement powers—the rights of workers to join organizations of their own choosing. In 1934, an amended and strengthened Railway Labor Act was rewritten with strong provisions banning company unions and protecting the rights of noncompany unions. The 1935 NLRA set up the federally administered National Labor Relations Board (NLRB) with broad powers to oversee the certification of unions and to penalize employers who did not accept the rights of employees to organize unions. In 1937, reversing its early precedents, the U.S. Supreme Court upheld the NLRA.

Whatever the substantive impact of this

stream of labor legislation,[4] its climax was the passage of a dramatic "prounion" bill opposed by a large majority of major capitalists and their organizations. Thus, the question of why it was passed at all and why it took such a seemingly pro-union form cry out for an explanation.

Explanations for the NLRA

One of the most prominent attempts to explain the events that culminated in the passage of the NLRA is that of Theda Skocpol and her collaborators (e.g., Finegold and Skocpol 1984; Skocpol 1980; Skocpol and Finegold 1982). Their views, which emphasize the autonomy of the state from societal forces are particularly useful to examine, first because they are consciously framed in opposition to other competing explanations[5] and second, because the state autonomy position is most opposed to the explanation I will argue is the best one. The views of the state autonomists may be summarized as follows:

1. On theoretical grounds, the state is most fruitfully viewed as potentially autonomous. Pluralists, elite and corporate liberal theorists, diverse types of Marxists, and others all err by wrongly viewing the state as dominated by, or the product of, various societal forces (Finegold and Skocpol 1984, Skocpol 1979, 27, 29; Skocpol 1980, 156, 199, 200; Skocpol 1985, vii, 4–6).

2. The New Deal period, or at least part of it—most especially those instances where labor legislation (particularly the NLRA) was passed—was one in which the state was actually quite autonomous from societal influences. All other theories fail (sufficiently) to take account of this autonomy (Finegold and Skocpol 1984, 169).

3. The argument is sustained in good part by an admirable attempt to eliminate contending explanations. Against pluralists it is argued that labor legislation was not passed because of its being supported by a multi-interest group reform coalition. Nor was it passed because of the leading role of President Roosevelt, as suggested by Schlesinger and others, since the president's priorities did not include the strengthening of unions. Neither the NLRA nor section 7(a) were the result of agendas by liberal corporate elites, as is asserted by elite theorists and corporate liberal theorists (Skocpol 1980, 166, 169), or certain capital-intensive segments of the business community, as is argued by Ferguson (1984). Still less was the passage of labor legislation a response to working class disruptions, as portrayed by Piven and Cloward (Skocpol 1980, 186–87). The passage of the NLRA was also not a response by procapitalist state managers to working class pressure or growing organizational strength, a response designed to control workers, as suggested by Block (1977). Labor was too weak to play such a role (Finegold and Skocpol 1984, 188, n. 42). In fact, in an argument that is viewed as giving the coup de grace to various structural Marxist positions, it is argued that labor legislation preceded the upsurge in union growth in the 1930s. Contrary to the claims of Block, Poulantzas, and others that such legislation would arise to control working class struggles, its passage stimulated and facilitated the growth of the union movement (Finegold and Skocpol 1984, 177; Skocpol 1980, 177–80, 185–86, 189; Skocpol and Ikenberry 1983, 160, 167).

4. Rather, the key is an understanding of the autonomous state structures, particularly the milieu in which Senator Robert Wagner operated and the political resources which he had developed. The role of Wagner and his advisers was heightened in part due to state incapacity resulting from the political and regulatory failure of the National Recovery Adminis-

tration (NRA). At a reasonably fluid juncture, when societal groups were weak (i.e., labor) or isolated (i.e., business) and liberal Democrats had gathered ascendancy in Congress as a result of the 1934 midterm elections, an unusually skillful senator with a history of legislative successes; a competent full-time research staff, including assistants with legal bill-writing talents; and a long-standing association with "progressive" reform groups—lacking the support of the president or his main advisors—with great perserverance carried the day, directing the passage of the NLRA (Finegold and Skocpol 1984, 177, 184; Skocpol 1980, 167, 180). Thus, the passage of the NLRA is "very much a tale of state and party" (Finegold and Skocpol 1984, 169).

Method

The question arises whether there is a viable method for deciding whether labor militance and radical organization played an important role in the passage of the NLRA or whether the state autonomy or some other model is more adequate. Stated another way, can there be rigor in establishing such broad social science explanations? I would suggest the following criteria, which are in principle not dissimilar from the approach taken by physical scientists when evaluating broad theories and hypotheses: (1) Does the explanation present a reasonable model that accounts for the most important outcomes and inputs, that is, does it fit the structure of the situation? (2) To what degree does the explanation lead its adherents to gloss over, omit, or distort important aspects of reality, that is, does it do violence to the facts? (3) How does it fare in regard to its competitors?[6]

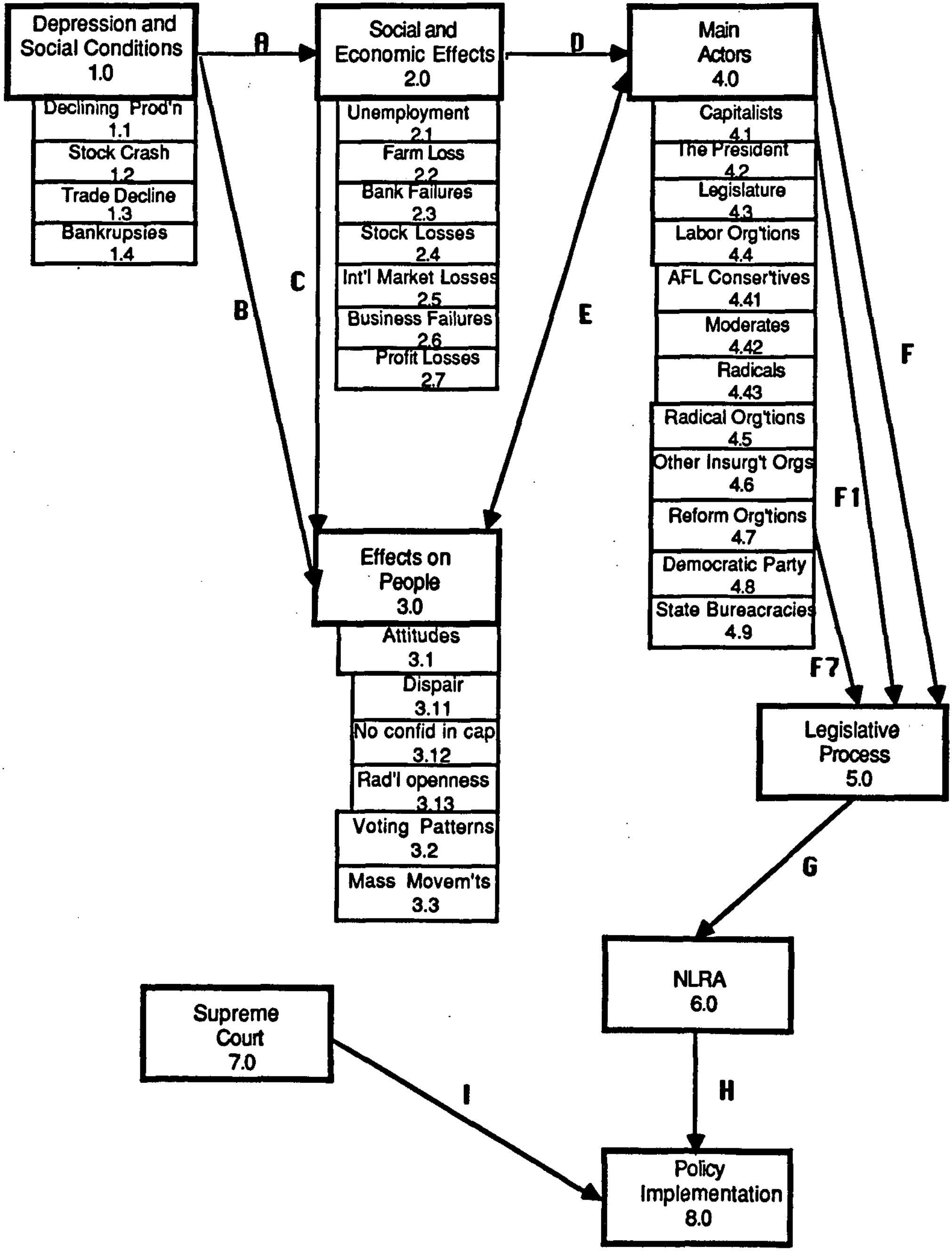
In trying to apply these interrelated criteria, my approach will be as follows: I will first suggest a diagram that attempts to model central features of all the various explanations. Then I will offer some conceptual distinctions that are blurred in most other analyses. In doing the above, I will be noting some central problems with the state autonomist explanation for the passage of the NLRA. Finally, I will provide an alternative model and argue for its superiority with respect to the above three criteria.

Analysis

A useful heuristic is to diagram the process by which New Deal labor legislation was passed, attempting to include all causal candidates. Different theories might then be distinguished by the causal arrows they emphasize, by the strength of particular arrows, by the complexities, subdivisions, and significant interactions at various places on the diagram. One possible diagram is presented in Figure 1. It should, of course, be emphasized that even such a detailed diagram must of necessity omit a large amount of material. Any particular theory would call for finer subdivisions in a specific area of causal emphasis and numerous additional lower-order causal arrows. Even more, however, the inclusion of many interactive and multifarious structural effects would make the diagram hopelessly complicated. Even so, the model is still not without its positive uses. Its use as a device to highlight what a particular theory deemphasizes, however, is less hindered by the above drawbacks. Most theories require the elimination (or relegation to marginal importance) of broad causal paths at the top levels of the diagram. Thus, the way that any particular theory simplifies the model will tell us quite a bit about the theory.

Virtually no one denies the existence or the importance of strong causal arrows A, B, and C. Levels 1, 2, and 3, however, are generally regarded as part of the background conditions. All agree that the Depression had major effects and a decisive impact on the majority of the popula-

Figure 1. Reasons for the Passage of the NLRA


Depression and Social Conditions 1.0
Declining Prod'n 1.1
Stock Crash 1.2
Trade Decline 1.3
Bankrupsies 1.4
A
Social and Economic Effects 2.0
Unemployment 2.1
Farm Loss 2.2
Bank Failures 2.3
Stock Losses 2.4
Int'l Market Losses 2.5
Business Failures 2.6
Profit Losses 2.7
D
Main Actors 4.0
Capitalists 4.1
The President 4.2
Legislature 4.3
Labor Org'tions 4.4
AFL Conser'tives 4.41
Moderates 4.42
Radicals 4.43
Radical Org'tions 4.5
Other Insurg't Orgs 4.6
Reform Org'tions 4.7
Democratic Party 4.8
State Bureacracies 4.9
B
C
E
F
F1
F7
Effects on People 3.0
Attitudes 3.1
Dispair 3.11
No confid in cap 3.12
Rad'l openness 3.13
Voting Patterns 3.2
Mass Movem'ts 3.3
Legislative Process 5.0
G
NLRA 6.0
H
Supreme Court 7.0
I
Policy Implementation 8.0

tion in the form of attitudinal changes, electoral realignment, and the development of mass social movements. The archetypical features of each theory involve their divergent analyses of the impact of level 4.0 actors on boxes 5.0 and 8.0 and of the interactions within box 4.0. Various theories may also be distinguished by the factors in box 4.0, which they assimilate into the background conditions, particularly of level 3.0.

Capitalist Influence

Just specifying that a causal arrow is important, however, is not always sufficient. This can be seen by a brief examination of what has been perhaps the most controversial arrow, that representing the influence (both positive and negative) of capitalists on the legislative process (box 4.1 and arrow F1). Influence may be distinguished not merely by its strength but by qualitative characteristics. I will distinguish between INFLUENCE1, providing the impetus for a bill to pass even if the content is not what the influencer wanted; INFLUENCE2, where the content is more or less what the influencer wanted, that is, the influencer dominates the content; INFLUENCE3, where the result of the bill, that is, the policy implementation, has the outcome that the influencer wanted. In addition, I might distinguish INFLUENCE4, the ability to block or control legislation, to force compromises that weaken the final act, or otherwise to control the agenda of decision making.[7]

In many instances, Skocpol and the state autonomists attempt to criticize various Marxist, corporate liberal, and elite theorists by arguing against an especially strong form of INFLUENCE3, when in fact the position only requires INFLUENCE1 or INFLUENCE2, or at most a weak form of INFLUENCE3. For example, the state autonomists argue that U.S. capitalists did not "control" the state in the implementation of the NIRA because the NIRA (which all agree was designed and implemented by business) had inadvertent effects that were to their disadvantage (Finegold and Skocpol 1984, 162–64). In particular, the NIRA failed to bring about economic recovery (Finegold and Skocpol 1984, 160; Skocpol 1980, 184) and through the vagaries of 7(a) stimulated large-scale labor-management disharmony and conflict (Finegold and Skocpol 1984, 160), eventually leading to the greater empowerment of workers.

Let us leave aside the empirical aspect of the above argument and concentrate on its logic. The claim that recovery was beyond capitalist control because state structures were not fully developed conflates the various types of influence. It is indisputable, of course, that capitalists to some degree influenced the passage of the NIRA in the form of INFLUENCE1 or INFLUENCE2. Skocpol supposes, however, that a claim that the capitalist class is dominant in the political system implies not only that they will control the implementation of public policy (perhaps a weak form of INFLUENCE3) but that they will also achieve their intended goals (a very strong form of INFLUENCE3). This argument glosses over the forms of capitalist dominance and seems particularly off the mark in the disputes with Marxists. Part of the ABCs of Marxist analysis is that crises are endemic to capitalism. While crises may be accelerated, exacerbated, or occasionally postponed and dampened by activities of the state, they are largely a product of the nature of capitalist society, hence beyond the "management" of the capitalist state (see, e.g., Mandel 1968, chap. 11; Sweezy 1964, chap. 8; for a sharp statement by Karl Marx with a comment by Frederick Engels, see Marx 1962, 118). Thus, the inability of the NRA to bring about recovery is a telling point only if one already accepts the potentially autonomous and omnipotent power of the capitalist state (i.e., a totally instrumentalist view of the state over society), which no view that emphasizes the importance of societal forces would likely grant.

The critique creates other straw men as well. Skocpol and her colleagues seem to read Marxism as believing that reforms (short of the abolition of capitalism) must always disproportionately benefit only capitalists since capitalists dominate the state (Finegold and Skocpol 1984, 162; Skocpol and Finegold 1982, 259).[8] The state autonomist's failure to make distinctions in types of influence leads them to miss crucial points and to ignore forms of influence that are less than absolute.

The problem also seems to arise in the state autonomists' dismissal of the claims of the historic corporate influence over various reformers. The state autonomists wish to argue that certain liberal reformers (e.g., Wagner, the Commons group in Wisconsin) and certain organizations (e.g., the American Association for Labor Legislation) had independent reform agendas that were neither controlled nor influenced nor coincident with those of major capitalists. To make this claim (about the nature of ties between levels 4.1 and 4.7 in the diagram), however, it is not enough to show that instances of INFLUENCE3 did not obtain in some or even all cases. The historical and conjunctural ties between capitalists and liberal reformers documented in the writings of Ferguson, Domhoff, and corporate liberal theorists are indeed extensive and impressive. It is, of course, important not to accept "guilt by association" arguments as constituting strong causal links. But it is also important for those who deny their importance to discuss the significance of links that appear, at least on the surface, to be far more than circumstantial. Skocpol, for instance, dismisses the question entirely with a misplaced analogy. She argues that discussing the connections would be similar to claiming that Marxist theory was sponsored by capitalists, since Engels, who gave Marx money, was the nephew of a capitalist (Skocpol 1980, 163). It is not unreasonable to distinguish capitalists and their progeny (assuming Engels may legitimately be described as such), acting as socially maverick individuals who use personal monies to support liberal or radical causes from corporate leaders and their representatives, acting in concert, expressing political and organizational goals for the advancement of their interests as capitalists?[9] And are not the linkages between activist organizations and their benefactors decidedly different in each case? All evidence suggests that we can and do make such distinctions. The proof of certain liberal reformers' independence from capitalists (i.e., lack of influence)—never really addressed by the state autonomists—is a prerequisite for the plausibility of their positions.

Similar problems arise in discussion of capitalist opposition to Roosevelt (INFLUENCE 4). Few participants in the New Deal debates seem to regard it as more than one type of phenomenon.[10] Capitalist opposition is a loaded term, representing a whole family of activities, not the single phenemonon implied in most discussions of the New Deal. There are a variety of degrees, with huge qualitative differences between various positions on the spectrum. We might easily distinguish between mild and strong forms of opposition to particular policies, opposition to the whole thrust of the New Deal reform agenda, active campaigning against New Deal politicians and the reelection of Roosevelt himself, and finally active work for his impeachment. It should be noted in passing that FDR's political skills and willingness to compromise kept certain opponents from moving too far along this spectrum for too long a period of time.

Although FDR was in fact opposed by a large majority of big businessmen on certain issues (the NLRA being perhaps the most notable), the good will, contacts, and lines of communication he had previously established kept business from going into more extreme forms of opposition.[11] Never did he face opposition of the type faced by the moderate quasi-socialist Upton Sinclair in the 1934 California gubernatorial campaign. No significant business groups demanded impeachment. Thus, the rhetorical descriptions of an embattled FDR facing a united, aroused capitalist opposition fail to distinguish analytically very different forms of capitalist opposition (INFLUENCE4), hardly doing justice to the actual situation.

The Nature of Labor Influences: Some Models

With these distinctions in mind, I now turn to the notion of labor influence (initially box 4.4 in Figure 1), itself in need of clarification. Since so little attention is paid in the New Deal debates to the possibility of labor influence, most models exist either implicitly or in undeveloped form.

One such model, disclaimed by all, involves a "powerful mass of organized workers" rising up and overwhelming "a united power elite position." Domhoff argues, for instance, that this is not how the NLRA was passed (Domhoff 1970, 249). Such a situation has virtually never happened. The working class seizing the capitalists by the throat and taking their stolen change from out of the capitalist money bags is a powerful illusion, but an illusion nevertheless. Many who attempt to downplay the importance of working class influence implicitly attack this straw man, thus deflecting attention away from the more likely forms of labor influence.[12]

A second model of labor influence on public policy is perhaps provided in Sweden. There, political parties (the Social Democrats and their allies) and several union federations (the Landsorganisationen and the Tjänstemännens Centralorganisation) represent the whole Swedish working class. These working class organizations formulate demands, negotiate with other peak political groups and employer organizations, and assist in implementing the final policies. In such a situation, the "influence" of labor organizations—if not workers themselves—is often easily ascertainable. Those who look for the clear imprint of labor on particular provisions of various bills implicitly advance this as their testable hypothesis. Such a model, however, rarely applies to situations of mass, newly organized worker insurgency; it clearly does not characterize the influence of labor in the Roosevelt era.[13] Both of these latter models might conceivably represent forms of INFLUENCE2 or INFLUENCE3.

A third model of labor influence is the Piven and Cloward (1979) disruption model, where capitalists respond to the spontaneous, unorganized, disruptive threats of the poor and underrepresented, clearly a form of INFLUENCE1. Criticism of their account plays a central role in Skocpol's analysis of the NLRA (1980, 186–87). This model, while fitting the general contours of certain aspects of the 1930s and coming closest to the view presented here, is also not without its problems. Its emphasis on spontaneity and disruptions leads one to overlook the role of highly organized radical organizations not only in organizing social protest but in tactical and strategic planning as well. Most threatening activity during the 1930s was actually highly organized and under radical leadership. In addition, it fails to understand the importance of the jockeying for position and influence between mainstream and radical groupings and its effect on public policy debates. Further, real patterns of influence during the 1930s were frequently more complicated, with the leading disrupters and their supporters sometimes vigorously opposing "their" legislation, particularly the NLRA.

If these three models were the only ones that could represent labor influence on the state, the case for labor influence on the New Deal would be difficult.[14] There exist other models, however, where influence is less direct, but still easily discernible.

More common, in fact, are concessions granted by a government in order to stem working class militance and organized radicalism. Sometimes the results are reluctantly supported or even opposed by the ruling classes, sometimes they are only an indirect response to insurgent demands. An example of the latter are the welfare state policies instituted by Bismarck in late-nineteenth-century Prussia, after a decade of antisocialist laws had failed to stem the growth of the world's largest Marxist working class party (see, e.g., Salvadori 1979, 21). To fail to see the agitation and struggles of the highly organized and disciplined German Social Democratic party as the moving force would be seriously to distort history. Likewise, while the Russian czar "gave" his constituents the Duma in 1906, few have failed to recognize it as a response to the massive working class and peasant insurgency and the well-organized forms of radical organization associated with the 1905 revolution (see, e.g., Lenin 1963).

The current process of unionization in South Africa is instructive in looking at models of working class influence, particularly with respect to the lag time between labor insurgency and its effect (INFLUENCE 1) on the public policy process. The most recent development of unions began in 1973 as economic expansion, fueled by huge rises in the price of gold (South Africa's major export), created an enormous demand for African labor (see MacShane, Plaut, and Ward 1984 for a highly informative account). Strike waves, coupled with informal demands, spread. The strikes in 1973, which received large international publicity, were particularly embarassing for many foreign companies. They also made profitable business difficult for the affected firms. Since open organization and leadership were illegal, formal bargaining could not occur. Workers engaged in guerillalike activities. As one manager stated, he was neither willing nor able "to negotiate with 1500 workers on a football field" (MacShane, Plaut, and Ward 1984, 51). Since repression would not work, certain capitalists developed a preference for orderly labor-management relations. One could search in vain for black worker input into the political negotiations that led to the 1979 enactment of the Wiehahn proposals legalizing black unions.[15] One could cite the importance of militant international support by protesters and unions, the role of foreign companies ostracized in their native lands, the speaking out of South African liberals, and the centrality of the rapidly expanding economy. But a refusal to recognize the preeminent role of the struggles of African workers (even though their strike rates tapered off several years before 1979) would be sorely mistaken.

These remarks are meant merely to indicate the complexities involved in the notions of labor influence and have not yet addressed the specific arguments against labor as a major factor in the passage of the NLRA. This task will be the burden of the next two sections.

Evaluating Labor Influence on New Deal Labor Legislation

The most important arguments by the state autonomists directed against the role of labor influence in the passage of the NLRA would seem to be the following: (1) The timing of the labor upsurges did not occur at the right times to have influenced labor legislation (Finegold and Skocpol 1984, 164); (2) Even if it had, labor was too weak to have influenced either 7(a) or the NLRA (p. 184); and most importantly, (3) The causality goes the other way, that is, the passage of 7(a) was the main stimulator of the labor upsurge from 1933 to 1953, and the NLRB largely enabled the growth of unionism from 1935–38 (Skocpol 1980, 181).

My plan in what follows will be to cast doubt on all three of these arguments. In addition, I will attempt to highlight the important effects of the interaction between labor militancy, social movements, and organized radicalism in the policy process. The last of the above three arguments will be examined first. The most decisive way to discredit a causal argument is to show that the effect actually preceded the supposed cause. Thus, the claim that union growth and activities influenced or caused the passage of labor laws may be disproved by showing that little or no activity or organization preceded the passage of the laws. Likewise, the argument that the laws caused the development of organization and activity may be easily disproved by showing that activity and organization (or its significant development) preceded the passage of the legislation. The dichotomies are rarely, however so clear-cut.

The question of the degree to which particular pieces of labor legislation may have stimulated, facilitated, or caused union growth and militancy is a complex one. A definitive proof would involve showing not merely that legislation preceded or even assisted union growth but that it would not have taken place otherwise—an extremely heavy burden. Moreover, the claim may be either weak or strong. A weak claim might assert that the law functioned symbolically to stimulate labor activity. This claim is difficult to disprove unless the law is shown to have been enacted after the development of the activity. It is similarly difficult to prove conclusively. A stronger claim is that the actual administration of the law either removed previous obstacles or facilitated and encouraged the activities in other ways. One must also leave room for the likelihood of joint causality, unless this possibility is ruled out by the temporal sequences. Whichever type of claim one makes, the examination cannot be dealt with lightly. Yet few analysts attempt to examine the question of labor influence in a rigorous manner. Finegold and Skocpol, for instance, make an extremely strong claim for the causal role of the NLRA, signed into law on 22 June 1935: "This act, and the independent National Labor Relations Board established to enforce it, facilitated labor organization and recognition, so much so that union membership grew from less than 4 million in 1935 to over 8 million in 1939 and doubled again during the war" (1984, 177).

An analysis of this claim will show a number of problems with the state autonomist argument. The best place to begin, however, is with certain questions of fact. The tremendous growth in labor union membership during World War II was hardly a doubling, going from over 10 million in 1941 to a little over 14 million in 1945 (see Table 1). Though I do not wish to be overly picky about these figures, it is important to set the record straight. Furthermore, as virtually all commentators agree, this growth during the war was not by and large due to provisions of the NLRA. Rather, the expansion of unions, the signing and maintaining of union shop agreements, and the growth in union membership, although under the auspices of the NLRB, took place according to rules established by the War Labor Board. Full union shop agreements were a condition for an employer's receiving a government contract, as long as unions honored no-strike agreements, something that some perspective observers argue ultimately weakened unions (e.g., Glaberman 1980; Lichtenstein 1982). Even more important, union growth during wartime often relies on favorable economic conditions (especially a tight labor market) and a desire for social tranquility and labor peace, thus making claims about its relation to particular state activities exceedingly complex.

The argument for the prewar period is especially dubious. Although the NLRA was signed into law in June 1935, the NLRB settled very few cases before it was upheld by the Supreme Court in the Jones and Laughlin case in April 1937. Until this time, virtually all employers refused to cooperate with the board. As can be seen from Table 2, only several thousand workers (less than 1% of the total) were organized under NLRB auspices before 1 July 1936, the end of the first full year of functioning under the NLRA. Union membership, 3 1/2 million in 1935, grew to slightly under 4 million in 1936. In 1937, in the aftermath of the Flint strike (28 December 1936–11 February 1937), General Motors, Chrysler, and Big Steel were unionized, along with hundreds of other companies. Within one month after the end of the Flint strike, 247 other sit-down strikes had taken place, involving almost 200 thousand workers (Preis 1965, 61). Union membership surged to over seven million by the end of the year; the dam had been broken with little help from the NLRB.[16]

If the NLRA (which only became truly functional at the tail end of the 1934–38 labor upsurge) and section 7(a) of the NIRA (which had no enforcement powers) were not administratively significant, it is still possible that they played an important symbolic, stimulating role. Legislation and small public policy changes have been known to have such effects on social movements (McAdam 1982, 50, 83–86, 108–9). It is certain however, that one cannot take the claims of conservative, moderate, or even sometimes left-wing union officials as proof of this. The passage of the Clayton Act in 1914, an act dubbed by then AFL president Samuel Gompers to be "Labor's Magna Charta," clearly played no such role (Gregory and Katz 1979, 159). The question of how to decide the symbolic significance of the NLRA is not an easy one. It would be foolish to argue that these pieces of labor legislation had no positive effect. My hypothesis is that they were one of a number of stimulating factors, certainly less important than successful, often highly publicized strikes.

The degree to which the state autonomists overemphasize the importance of

Table 1. Union Membership, 1897–1948 (Selected Years)

Year	Number of Members (in thousands) Wolman Series	BLS Series
1897	447	—
1900	868	—
1901	1,125	—
1904	2,073	—
1912	2,452	—
1914	2,687	—
1916	2,772	—
1917	3,061	—
1918	3,467	—
1919	4,125	—
1920	5,048	—
1921	4,781	—
1922	4,027	—
1923	3,622	—
1929	3,443	—
1930	3,393	3,401
1931	3,358	3,310
1932	3,144	3,050
1933	2,973	2,689
1934	3,609	3,088
1935	—	3,584
1936	—	3,989
1937	—	7,001
1938	—	8,034
1939	—	8,763
1940	—	8,717
1941	—	10,201
1942	—	10,380
1943	—	13,213
1944	—	14,146
1945	—	14,322
1946	—	14,395
1947	—	14,787
1948	—	14,319

Source: Wolman Series is taken from Leo Wolman, *Ebb and Flow in Trade Unionism* (New York: National Bureau of Economic Research, 1936). BLS Series is taken from Goldfield 1989.

Table 2. NLRB Election Results, 1935–48

Year[a]	Elections	Elections Won	Eligible Voters	Valid Voters	Union Voters
1935–36	31	18	9,512	7,734	4,569
1936–37	265	214	181,424	164,307	113,484
1937–38	1,152	945	394,558	343,587	282,470
1938–39	746	574	207,597	177,215	138,032
1939–40	1,192	921	595,075	532,355	435,832
1940–41	2,568	2,127	—	729,915	589,921
1941–42	4,212	3,636	1,296,567	1,067,037	895,254
1942–43	4,153	3,580	1,402,040	1,126,501	923,169
1943–44	4,712	3,983	1,322,225	1,072,594	828,583
1944–45	4,919	4,078	1,087,177	893,758	706,569
1945–46	5,589	4,446	846,431	698,812	529,847
1946–47	6,920	5,194	834,553	805,474	621,732
1947–48	3,222	2,337	384,565	333,900	256,935

Source: All figures are from NLRB annual reports for the appropriate fiscal year.

[a] NLRB fiscal years begin 1 July of first year and end 30 June of second year.

symbolic legislative actions like the NLRA and of institutional bureaucracies, ignoring the historical context in which they exist, is suggested in what happened after the enormous labor upsurge of 1937. By the next year, with a legally reaffirmed NLRA, union growth had begun to slow, then stagnate. If one were concerned largely with correlations and temporal sequences and with attempts to always find primary causes in state activities rather than with an attempt to understand the deeper causes, one might argue that the NLRA itself was responsible for inhibiting union growth. Such an argument, however, would be perverse, attributing more instrumental causal import to public policy than is reasonable in this case. Skocpol, in her haste to find state-centered explanations, attributes this stagnation (less than a year after the NLRA actually began functioning) to the shift in political winds (Weir and Skocpol 1985, 112).[17] It is at least as likely, however, that the causal arrows point in the other direction. The 1938 economic downturn, of course, bears some responsibility for slowing unionization. The major factor, however, seems to have been the widening split between the AFL and the CIO, described by some observers as a "civil war." This split allowed the Right and corporations to regain the offensive (Davis 1986, 69–72). Without this split—and consequent AFL attacks on the NLRB, coupled with AFL political support for conservative congressmen—it is unlikely that unions or the NLRB would have been as vulnerable as they were (Gross 1981, 73–85, 91–108, esp. 263). Thus, one must conclude that the NLRA, while it may have facilitated some union growth, was probably not a major cause of the tremendous union upsurge from 1934 to 1938.

An Alternative Model

Further objections to the notion of labor influence on the passage of the NLRA will be dealt with in the context of my discussion of an alternative model. The purpose of this model will be to explain why the NLRA was passed and the important role that labor influence played. In short, my model may be outlined as follows:

1. New Deal labor legislation was a

result of interaction between labor movement growth and activity, the increasing strength and influence of radical organizations, particularly the Communist party, liberal reformers with both immediate and historical corporate ties, and government officials (or state managers) with primary concern for preserving social stability and assuring the continued electoral success of the Roosevelt-led Democratic party. Thus, the alternative theory, in contrast to the others, stresses interactions between actors on level 4.0 in Figure 1, the impact of these interactions on the public policy process, and the special importance of boxes 4.4, 4.5, and 4.6.

2. While the particular content of virtually all the New Deal legislation was a direct, though evolving, product of long-standing reform agendas, the impetus for passage, some features of the bills, and the immediate reasons why the legislation was passed (i.e., why a large number of senators and congressmen voted for it, why business did not oppose it in a more extreme manner, and why the president signed it) was a direct result of the broad labor upsurge, conflicts within the labor movement, and the growing influence of radicalism.

3. The historic ties between reform organizations, reform politicians like Robert Wagner, and various business groupings and the ties of all of them at various times to AFL leaders are central to an understanding of the role played by reformers. Reformers and reform groups had at best a limited semiautonomy with little independent power. Their main power was derived from their connections to certain capitalists who often sustained their activities or from their acting as brokers for conservative and moderate leaders of the labor movement. Their effective power and influence tended to rise with the increased power and influence of labor. They had narrow room for maneuver, usually being buffetted by events and social forces.[18]

4. New Deal labor legislation was neither so radical nor so out of context from previous legislation as many post–World War II analysts have portrayed it. First, there was a long history of precedents on which to draw, as Wagner and other supporters continually emphasized (for Wagner's remarks on 11 March 1935, see NLRB 1985, 1408; also see, e.g., Bernstein 1950). Second, the legislation was not so unambiguously prolabor; it was criticized at the time by a large number of radical and liberal organizations and individuals before its passage.[19]

5. The key to an understanding of the influence of labor is two aspects of the development of the labor movement in the 1930s. *First,* the labor movement was much broader than the movement to organize unions. Well before the labor upsurge at the workplace became widespread, militant working class movements of the unemployed and African-Americans were mobilized in large numbers. These activities were paralleled by sympathetic and supportive movements of students and intellectuals. There were also highly influential left-wing political parties that received and gave significant labor union support; these included the Minnesota Farmer-Labor party (Valelly 1989), Upton Sinclair's End Poverty in California (EPIC) organization, the American Labor party in New York (Waltzer n.d.), Wisconsin's Progressive party, other statewide organizations, and perhaps hundreds of local labor parties (Davin and Lynd 1979–80). These movements and organizations added a breadth and broad-based support to nascent labor struggles, providing especially fertile soil for the Communist-dominated left-wing. A more amorphous and even broader milieu gave additional sustenance to protest, including Huey Long's Share the Wealth movement (which before Long's assassination gave Roosevelt cause for great political concern about the Left) and the Townsend movement with its millions

of members and perhaps also the early, diffuse, antirich, eventually rightist Coughlin movement (Brinkley 1983). To look at union growth outside of the environment that nourished it is to fail to grasp the phenomenon fully. These social forces expressed themselves in a variety of ways, including their translation into electoral victories for FDR and New Deal insurgents in Congress, thus providing a more responsive legislative environment to be influenced directly.[20] *Second,* throughout the 1930s there was tremendous conflict (largely ideological) within the labor movement. There was a growing split between conservative AFL leaders (with their historic ties to reformers and liberal capitalists) and the Left, which for the first half of the decade was becoming an increasing threat to the former.

This descriptive model for the passage of the NLRA differs in fundamental ways from that of the state autonomists. It not only pays far greater attention to the influence of societal forces on the political arena, but also looks at the state-society interactions that help explain both the periods of greater influence of certain governmental figures like Robert Wagner and certain of the characteristics of the final bill.

While all five parts are important for the model, the central burden of such a theory is the demonstration of three linkages: (1) that there existed a strong and important connection between the radical labor upsurge and the broader social movements; (2) that a growing political conflict was taking place within the labor movement, whose balance of power was beginning to shift from conservative leaders and organizations to more radical groupings; and (3) that both these phenomena had a central impact on the labor reform process.[21] I will attempt to demonstrate these linkages in a preliminary fashion by examining the factors that led to the passage of the NLRA.[22]

How the NLRA Came To Pass

The first task will be to show the strength of early New Deal social movements and their impact on, and support for, the more slowly emerging labor movement. In this context, I will also begin to outline the linkages mentioned above. My central caveat will be that standard modes of evaluating organizational and movement strength by membership figures, electoral impact, or legislative influence are even more problematic during times of popular insurgencies than they are during more normal times. The first most dramatic mass response to the Depression came from the unemployed.

The Unemployed

Unemployment quickly became the dominant political focus at the beginning of the depression. Millions of people roamed the country looking for work. Large shantytowns grew inside and outside major cities. State governments approached bankruptcy with relief efforts that scarcely scratched the surface of the problem (see Bernstein 1960, 287–311, 416–36, 456–74 for detailed descriptions).[23] Protests of the unemployed from 1930 to 1932 were often massive and militant. No serious commentator doubts that they were virtually all radical-led, largely by open communists. On 6 March 1930, well before the passage of any of the new labor legislation, over one million people demonstrated across the country under Communist Party (CP) leadership against unemployment (Klehr 1984, 32–34). Harvey Klehr describes massive funeral rallies led by the communists in key cities around the country. In New York City, in January 1930, 50 thousand attended the funeral for a party activist killed by the police. A similar funeral in Detroit in 1932 for four party activists killed by the police at a protest march on Ford's River Rouge plant was attended by 20–40 thousand people: "Above the coffin was a large red banner with Lenin's picture" (p. 59). Perhaps the high point of such activity was in Chicago. In one incident in 1931, five hundred people in a Chicago southside African-American neighborhood brought back furniture to the home of a recently evicted widow. The police returned, opened fire, and three people lay dead. The coffins were viewed, again under an enormous portrait of Lenin. The funeral procession with 60 thousand participants and 50 thousand cheering onlookers was led by workers carrying communist banners: "Within days, 2,500 applications for the Unemployed Councils and 500 for the Party were filled out" (pp. 332–33).[24] From all indications, these protests, as well as the political character of their leadership, often making the front page news, did not fail to leave deep impressions on many people in positions of power, as well as on the more disadvantaged members of the citizenry.[25]

Other Protests

The struggles of farmers likewise developed widespread militancy, often involving as many as tens of thousands in direct actions. Activities included the withholding of produce from the market because of low prices and the stopping of banks from auctioning mortgage-defaulted properties, sometimes by armed "penny sales." Communist and radical influence here, while not nearly as extensive as among the unemployed, was far from negligible (Klehr 1982, 139–146). In the early stages of the Depression, when virtually all farmers were desperate, militant farm organizations, particularly the Farmers Holiday Association, were sympathetic and supportive to union struggles (Shover 1965; Valelly 1989).

Large-scale protests by students, often under CP influence, began in the early 1930s (Klehr 1984, 307–23). At the same time, thousands of intellectuals and artists, including a number of the nation's most prominent, publicly declared their allegience to communism. In numerous instances, these intellectuals formed support committees and publicized working class grievances widely (Cochran 1977, 54–57; Klehr 1984, 70–84).

Perhaps nowhere was the upsurge so militant and the rapid influence of communists as dramatic as it was in African-American communities. In 1931, the CP took initiative in a case that was to gain it major political leadership among African-Americans throughout the country. The case was that of the Scottsboro boys, nine African-American youths seized from a freight train in rural Alabama, accused of raping two white girls who had been riding with them on the same train. The Scottsboro defense laid the basis for the large-scale influence and recruitment of African-Americans of every stratum throughout the United States. Defense activities involving significant numbers of whites, as well as many blacks, were numerous, widely attended, broadly supported, and well publicized. These activities and the reputation gained by the CP as a reliable defender of black people gave it entree and influence among highly concentrated African-American industrial workers, including in such important places as the Birmingham steel mills, the Briggs automobile plants in Detroit, and the Ford River Rouge plant, then, as now, the largest plant in the United States (Goldfield 1980, 1985; H. Haywood 1978; Honey 1986; Hudson 1972; Huntley 1977; Keeran 1980; Meier and Rudwick 1979, 1982; Naison 1983).

This atmosphere of social protest and radicalism was nourished and gained recruits from the broader milieu of unorthodox movements, and local-and state-level labor parties. Within this environment the labor movement began to assert itself in the nation's workplaces.

The Workplace

Throughout the 1920s, groups of communists had organized themselves in industrial plants throughout the country. In many of the ununionized industries, they were the only organized forces, occasionally having the broad sympathies of their fellow workers on the basis of clandestinely published shop papers (Cochran 1977, 43–81, esp. 63–64; Keeran 1980, 39–44; Marquart 1975, 33–35). In the fur and leather industries, centered primarily in New York City, the union was openly led by communists (Foner 1950). In a number of other industries (including mining, textile, and some maritime sectors), they led and participated in large-scale, though generally unsuccessful, strikes. In early 1933, however, months before the passage of NIRA, the CP, along with members of the Industrial Workers of the World and independent radicals, led a series of successful strikes at Briggs in Detroit that were to help them establish early hegemony and respect in the auto industry (Keeran 1980, 77–95). It is not always clear from reading historical accounts how much to generalize from the reactions of individual capitalists and business organizations to the labor struggles of the early 1930s. It is somewhat easier to see the distress of established AFL leaders at the degree of opposition growing within their own organizations and their eclipse among newly organizing workers.

All the central aspects of the alternative model which were magnified greatly in the wake of the 1934 labor upsurge, are belittled in importance by Skocpol (1980, 187). The long, continuous decline in union membership from 1920 to 1933, was reversed in 1934, as union membership increased by 20%, rising by over 600 thousand members (Wolman 1936, 16). Strike statistics took an extraordinary leap. But these are mere incidental statistics, which fail to convey the depth of the explosion. One does not even have to rely on enthusiastic radical accounts, accurate as they may be. Irving Bernstein, for instance, writes over three decades later of 1934:

> A handful of years bears a special quality in American labor history. There occurred at these times strikes and social upheavals of extraordinary importance, drama, and violence which ripped the cloak of civilized decorum from society, leaving exposed naked class conflict. Such a year was 1886, with the great strikes of the Knights of Labor and the Haymarket Riot. Another was 1894, with the shattering conflict of Eugene Debs's American Railway Union against the Pullman Company and the government of the United States. Nineteen thirty-four must be added to this roster.
>
> In the summer of that year Eric Sevareid, who covered the great trucking strikes for the *Minneapolis Star*, returned home to find his father on the screened porch. The elder, a Minneapolis businessman, was reading the headlines and his face was pale. "This," he said, "this—is *revolution!*" (Bernstein 1969, 217).

Three labor struggles, if not revolutionary, were certainly deep social unheavals: in the 1934 conflicts in Toledo, Minneapolis, and San Francisco, highly organized workers were victorious. All three struggles were led by avowed revolutionary groups and linked previously mobilized, separate constituencies. In Toledo, the working class and organizations of the unemployed formed a major alliance, with tens of thousands of radical-led unemployed workers battling scabs and National Guardsmen to a standstill, rescuing a defeated strike. In San Francisco, even the conservative AFL unions were drawn into the general strike. And in Minneapolis —a previous open shop, low-wage citadel —not merely the unemployed AFL unions and Farmer-Labor party organizations but militant farmers under the banner of the Farmers Holiday Association joined in the struggles of the Minneapolis working class (Dobbs 1972, 68). These great battles stimulated and encouraged workers throughout the country, both directly and indirectly, well after the successful strikes had ended. After the 1934 San Francisco general strike, the longshore and maritime industries along the whole West Coast remained aflame with militancy, largely under communist leadership. The Trotskyist-led triumph in Minneapolis laid the future basis for the successful organization of over-the-road truck drivers through the Midwest. And in auto organizing outside of Detroit by communist-led shop groups and in Detroit by the radical Mechanics Education Society of America was greatly accelerated (Keeran 1980, 103–7, 121–37; Preis 1964, 19–33). As happened in General Motors after the 1936–37 Flint strike, workers engaged in numerous unofficially sanctioned (and undoubtedly officially *unrecorded*) job actions, gaining working conditions that employers never would have conceded in the previous bargaining. Most likely, these strikes increased the fear among the rich of revolution. In all probability, they made politicians committed to capitalism somewhat apprehensive. For AFL leaders, however, these strikes must have had the appearance of the grim reaper. They signified the existence of an emerging mass-based labor movement led by radicals, completely outside their control. This movement threatened to overwhelm them even inside the confines of their own organizations (Davis 1986, 56–57).

The Response to the Labor Upsurge

The most reasonable hypothesis to account for the passage of the NLRA is that labor militancy, catapulted into national prominence by the 1934 strikes and the political response to this movement, paved the way for the passage of the act. Having talked about the insurgency, I shall dwell on the response to it.

First, the labor insurgency, with its accompanying conflict and violence caused by intransigent company resistance, had reached proportions truly alarming to the economic and political elites. To interpret this concern as having abated due to downward fluctuations in strike statistics in early 1935 is to miss a central aspect of political and social reality. The 1934 labor revolt, for instance, was the dramatic centerpiece of a highly effective speech given by Robert Wagner at the House Labor Committee Hearings on 13 March 1935 (NLRB 1985, 2498) and on the Senate floor on 7 May 1935 (p. 2342). During the 1934 hearings prior to the 1934 successful, radical-led strikes and after, there were many who predicted increasing unrest. These included not only labor leaders such as Sidney Hillman of the Amalgamated Clothing Workers and John L. Lewis of the miners but many other public and private figures. On 29 May 1934 Senator LaFollette talked about growing labor unrest and "this impending crisis . . . which will bring about open industrial warfare in the United States" (p. 1202). On the same date Representative Connery noted, "You have seen strikes in Toledo, you have seen Minneapolis, you have seen San Francisco, and you have seen some of the southern textile strikes . . . but . . . you have not yet seen the gates of hell opened, and that is what is going to happen from now on" (p. 1150). And contrary to the perceptions of Skocpol and other state autonomists, most observers saw only an increase of labor "strife" throughout the spring of 1935.[26] On 13 March 1935 Wagner, for example, the state autonomists' archetypical state reformer, spoke of the "rising tide of industrial discontent" (p. 2487). These sentiments and the fear of even greater labor struggles are echoed by virtually every commentator during the spring of 1935. No opponent in the hearings or on the floor of Congress ever rises to suggest the opposite or even that the descriptions are overdone. Even William Green, the ever-cautious head of the AFL, attempted to use labor unrest to political advantage. On 23 May 1935 the nations' presses reported that he addressed a rally of 25 thousand workers in Madison Square Garden in New York City, with another 25 thousand standing outside. Here Green threatened (in a manner reminiscient of his performance over the 1933 Black bill) a national general strike if the NLRA was not passed. Though this may have been merely puffery and bluster on Green's part, the results of his threat were different in 1935. This time, on the next day, according to the *New York Times*, 250 thousand New York City needle trade workers quit work early in support of Green's demand.

The response to this increasing unrest was, of course, not uniform. Differing perspectives emerge quite sharply in the discussions of the NLRA. Most critics, as well as supporters, recognized that the NLRA was designed to empower AFL unions. Large numbers of employers and their organizations opposed the NLRA because they believed it would strengthen or give unfair monopolies to the AFL. This position was perhaps typified in the remarks of James A. Emery, the general counsel for the National Association of Manufacturers. On 26 March 1934 Emery argued at the Senate hearings that the bill would issue a monopoly to the AFL: "It is a deliberate step toward a Nation unionized by the act of Government" (NLRB 1985, 428; Donald A. Callahan of the American Mining Congress made a similar statement on 27 March 1935, [p. 1999].) And there were, of course, more extreme opponents, whose views of the NLRA ranged from "pregnant with class antagonism" (e.g., Morris Torrey, the Employers' Association of North Jersey on 28 March 1934 [p. 512]) to "[based on] Karl Marx's philosophy of economics" (Guy L. Harrington of the National Publishers' Association on 22 March 1935 [p. 1662]). Some other larger employers, however, although opposed to the NLRA on various grounds, seemed more sympathetic to empowering the AFL and diffusing strikes. Members of this tendency worried that the NLRA would lead to more conflict and provide greater power to communists. These views are perhaps represented in the statements of Henry I. Harriman, the president of the U.S. Chamber of Commerce on 29 March 1934 (pp. 529–33). Harriman and others pushed for certain amendments, but did not oppose the NLRA in principle.[27]

There were also large numbers of individuals and groups from the Left who opposed the bill. It was viewed by many, including the ACLU, as potentially restrictive of the right to strike (e.g., the Senate testimony of John H. Gray on 22 March 1934 [NLRB 1985, 336]; see also Daniel 1980) and too biased toward the allegedly procompany AFL (e.g., the testimony of Francis Dunne of the CP-initiated Trade Union Unity League on 9 April 1934 [p. 1010]). Leftist groups in general (with the exception of the Socialist party, which supported the NLRA) were suspicious of any expansion of government authority to intervene in labor-management relations. The NAACP and the Urban League also voiced opposition, unless the bill were to include guarantees for the rights of African-American workers. These groups and individuals, while occasionally representing broad constituencies and significant popular impulses, had little direct political influence in 1934 and 1935 (i.e., they had INFLUENCE1 but not INFLUENCE2).

The dominant political response to the increasingly powerful labor upsurge between 1933 and 1935, however, was to support the NLRA. The virtually unanimous opinion among New Deal Democrats and progressive Republicans (the overwhelming majority in both Houses after the November 1934 elections) was that government regulation was necessary to constrain, limit, and control the increasingly militant labor movement. This position, a central feature of the preamble and section 1 of the bill, runs like a bright yellow thread through the hearings and floor debates of both Houses.[28] Representative Withrow of Wisconsin on 18 June 1935, the last day of floor debate in the House, argued "As has been said by the gentleman from Pennsylvania [Rich], strikes have been prevalent in this country during the last two years. . . . The passage of this legislation is the only cure for the labor difficulties which have been characteristic for the past few years" (NLRB 1985, 3132). And as Representative Sweeney of Ohio predicted on the same date, "Unless this Wagner-Connery dispute bill is passed we are going to have an epidemic of strikes that has never before been witnessed in this country" (p. 3168).

The question naturally arises why repression and resistance were not considered a live option by political leaders in 1934 and 1935. In other words, what was attractive about mobilizing government support for moderate unionism by reducing employer capabilities for resistance? Two factors were important in making this latter option the most compelling one. The first had to do with the general social and political unrest in the country. The second (and perhaps most immediately important) had to do with the rapidly growing strength of radicalism in the working class in the United States. As Frances Perkins notes over a decade later, it is easy for even a former participant to forget the atmosphere of political crisis that existed in the first half of the 1930s (Perkins 1946, 182). To some political and economic elites, the possibility of revolution against the capitalist system was quite real. (Lipset [1983, 274–79], for instance, presents evidence for "the leftward shift in public opinion during the 1930s" and the generally large-scale influence of radicalism in this country.) Many diverse references are available indicating that individual executives and some politicians in the United States during the 1930s feared a revolution (e.g., Karsh and Garman 1957, 83; Leuchtenburg 1963, 25). Adolph Berle, Donald Richberg, and others thought government reforms were immediately necessary to avoid more radical demands and activity. Perkins for instance, was urged in the spring of 1933 by Berle, her close friend, to leave Washington before "widespread violence" broke out. (See Lowi 1969, 217 for Perkins's account; see also Richberg's statement at the Senate Hearings on 6 January 1935 [NLRB 1985, 1290–93]; William Green's on 14 March 1935 [p. 1477]; Connery's on 18 June 1935, [p. 3289]). As Schlesinger states, "It was now not just a matter of staving off hunger. . . . It was a matter of staving off violence, even (at least some thought) revolution. Whether revolution was a real possibility or not, faith in a free enterprise system was plainly waning" (1958, 3).

Into this social milieu, tempered by the raised, then shattered hopes of the country in the NRA in general and section 7(a) for workers, burst the 1934 strikes, led by avowed revolutionaries, magnified by the linkages to other insurgent consistencies, the aftermath of which promised continued struggles on even broader scales. The 1934 events hang like a veil over the NLRA hearings and floor debates not just in 1934 but through the spring of 1935. To deny the impact of the 1934 upsurge is to miss a central aspect of reality. Theories that lead their adherents to overlook or slight these labor struggles must be deemed deficient. References to the dip in recorded strikes during early 1935 (see Finegold and Skocpol 1984, 180–81 for an attempt to dismiss the causal importance of the 1934 upsurge with such a reference), even if they did reflect a temporarily lowered level of insurgency, would tell us little about the threat that the 1934 strikes made, both to corporate elements and to AFL leaders as well.[29] Those who pushed for the NLRA placed many of their arguments within this context.

There was also deep concern that the rising level of conflict caused mainly by the intransigence of most employers, was in good part the reason for the growing strength of labor radicalism and the relative weakening position of the AFL. The AFL was caught in the early thirties between a rock and a hard place. On the one side they were attacked and disowned by militants whose tactics they would not support, and on the other they were beaten back by recalcitrant employers who were not amenable to persuasion and moderate pressure. The complaint that employer hostility and government complicity were helping revolutionary labor groups may be heard repeatedly from top conservative AFL leaders, including William Green and John Frey, president of the AFL Metal Trades Department. This sentiment is voiced sharply by Frank J. Dillon, the director of auto organizing, personally selected by Green; Dillon had played a major role in undermining the then-upcoming auto strike in 1934. On 28 March 1935, at the House Hearings, Dillon stated,

> It is significant to here record the fact that Communists and communistic theories are more prevalent and substantially stronger among employees within the auto industry now than 1 year ago, constituting an actual menace to the future of the industry and a challenge to our form of government. It is my humble judgment that this feeling of bitterness, hatred, and resentment now so prevalant among auto workers is the direct result of management's failure to genuinely conform to the spirit and intent of section 7(a) of the NIRA. (NLRB 1985, 2725)

However self-serving these claims may appear, the analysis has the ring of truth, reflecting the AFL loss of the emerging autoworkers' movement to the Left and the future UAW-CIO leadership.

Thus, there began to emerge a growing concensus among liberal politicians that the best way to preserve order, prevent high levels of strike activity, slow the spread of communism, and diffuse serious challenges to the capitalist system was by creating a government-supported legal environment where moderate forces, particularly the AFL leadership group, were protected and not so disadvantaged as at present. Hence, certain moderate forces were in favor of the NLRA because they thought it would strengthen the hand of the AFL. As Lloyd Garrison, the chairman of the pre-NLRA National Labor Relations Board argued on 15 March 1935, "I am for it as a safety measure, because I regard organized labor in this country as our chief bulwark against communism and other revolutionary movements. . . . I think that those employers who are out to strangle organized labor, are simply playing into the hands of the extremists" (NLRB 1985, 1505).

To many in 1935, it did indeed seem that a specter was haunting the United States. In a sentiment echoed by diverse people during the NLRA debate, Representative Connery on 4 April 1935 responded to the testimony of Dr. E. R. Lederer, who represented the Petroleum Industry's opposition to the bill:

> Dr. Lederer, I believe personally that the big corporations, like the Standard Oil Company, the Shell Oil Company, and these big textile industries, and the automobile industry, are very short-sighted. . . . They regard us as enemies of the employers, as actually being inimical to the employers, when we are not. What we are trying to do, Dr. Lederer, is to save those corporations from communism and bloodshed, and, Dr. Lederer, the Government wants them to give labor of the United States a fair deal. The American Federation of Labor, to which you referred, is the bulwark that is holding back communism in the United States among the workers, by having them in organized units where they can be self-respecting American citizens and have a chance to bargain collectively for their rights. They are keeping men in line who, if they did not have that union, would say 'All right, we get no protection from the government; we are slaves to our employers. Let us go out like they did in Russia and let us turn the government upside down and take the money away from these fellows. . . . I am surprised that the big employers cannot see that, and do not regard the committee as their friend rather than an enemy (NLRB 1985, 2789).

And so the NLRA passed, first the Senate, then the House of Representatives—overwhelmingly. It is, of course, not necessary to argue that everything turned out exactly the way it was planned in the 1930s. It certainly took a good many years before labor insurgency was tamed and the influence of radicalism was checked. A convincing argument, however, can be made that the strategy was ultimately successful (Goldfield 1987; Lynd 1987; Rogers 1984; Tomlins 1985).

Conclusion

The foregoing analysis has suggested that the state autonomy model, which searches for primary explanations of politics in "states and parties" seriously slights the importance of the influence of labor militancy and radical organization in the passage of the 1935 NLRA. An alternative model examining the state and parties in conjunction with, and in the context of, the influence of key class actors is better able to account for the bill's enactment.

Models, however, do not get "proved" or disproved by appealing to this or that fact or situation. A good model may be modified, incorporating new material and supporting hypotheses. Rather, a model is to be judged by how useful it is in organizing the facts, in accordance with the three criteria suggested earlier.

First, it was argued that an adequate model must be able to account for all the important inputs and outputs, reasonably fitting the structure of a situation. The model presented here is able to take the strength and impact of labor militancy, broad social movements, union influence within the legislative process and governmental circles, and radical organization into account and suggest the mechanisms by which they influence important actors and politics in general.

Second, the model gives significant weight to important social forces and tries to leave room for varying types of influence and interaction of these forces with the state. It is important to evaluate public policy on its own terms. It may be truly momentous, with great social impact; a codification of already existing practice; or largely irrelevant.[30]

Third, the model outperforms the state autonomy model, which is led to slight systematically such factors as labor militancy, social movements, union lobbying, and radical organization and to distort reality in important ways—giving highly aggregated statistics where more disaggregated ones suggest a different story; belittling the importance of the 1934 strikes, finding a sharp decline in labor militancy in 1935, where one sees at most a rather normal cyclical dip; failing to examine or describe the perceptions of the upsurge from the standpoint of key policy makers and legislators; and describing little of the radical organizations and atmosphere that others (e.g., Lipset 1983) have seen as so central a part of the 1930s landscape. In short, the state autonomy model pushes its adherents to abandon the social context of political events. These sins of omission and commission flow from the ill-begotten attempt to impose the state autonomy model where it does not belong.

An important comment must be made about the significance of conjunctural factors, which are so often stressed by Skocpol and other state autonomists. It is often made to seem as if one must choose between an emphasis on these factors and social forces. This, however, would be a misformulation of the problem. Conjunctural factors are always important. Without a line to protect them, no great quarterback or running back ever scores a touchdown. Stephen Gould (1988) makes this point sharply in his discussion of the lucky breaks and circumstances of Joe Dimagio's hitting streak. Yes, business was discredited, and the NLRA did come up at precisely the time that the majority of capitalists had abandoned FDR politically. In Congress the November 1934 elections had removed the Republicans and much of the Right. These factors were important causal forces in making the state responsive to the developing labor struggles and the growing strength of labor radicalism but certainly not sufficient in themselves. Without the 1934 or some similar upsurge, it is unlikely there would have been an NLRA. Liberal politicians, reform groups, and the small number of faithful New Deal executives did not develop their political hegemony on their own. They gained strength and influence as a result of broad social forces. Certainly, it is conceivable that if all the conjunctural factors had been different the 1934 labor upsurge and its consequences would have been insufficient to force the passage of the NLRA. But then the labor movement might have continued to develop, perhaps a little later, perhaps more violently, certainly in a more radical political direction.[31]

Labor influence was central to the structure of the political situation in 1934 and 1935, both because of the growing strength of its insurgent and disruptive activities and because of the growing strength of highly organized radicalism. Because of this latter development, repression by companies (often aided by local police and state-directed National Guardsmen) was not an unproblematic option; it was already discrediting the more collaborationist wing of the AFL and giving greater legitimacy to radicalism. Liberal politicians (many with longstanding direct and indirect ties to corporate reform groups) thus reacted with a combination of sympathy and alarm at the growing labor upsurge with all its complexities.

If labor militance, social movements, and radical organization have had a major impact on public policy and on the general politics of the 1930s, no model of the modern state or research program for studying it makes it difficult for the conscientious investigator to uncover this impact is adequate. Thus, the state autonomist approach and others that slight these factors must be judged inadequate. If the politics of class and social protest have been important in the past, they may be potentially important for the future. More attention should be paid in the political science profession to the study of these factors in the 1930s and the 1960s. The periods of relative quiet as well as the crescendos may be worth explaining, since they too may prove central to understanding what is essential about U.S. politics.

Notes

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1. By class legislation I refer primarily to labor laws and social welfare legislation. The New Deal era, of course, was the reforming period (1933–38) of President Franklin Delano Roosevelt's administration.

2. Analysis of the role of such movements in Europe has been made by Charles Tilly (1978) and Sidney Tarrow (1989) among others.

3. The conditions of labor organizations are documented in an extensive literature. For representative material see Bernstein 1960; Foner 1962–82; W. Haywood 1929; Perlman and Taft 1935; and Preis 1964. For extensive documentation during the 1930s see the LaFollette Hearings, U.S. Congress 1937–41, pts. 1–75. A particularly vivid, informative historical novel is Giardina 1987. For the legal background see Gregory and Katz 1979.

4. The NLRA was criticized by many as antilabor even before its passage. Such critics included the American Civil Liberties Union; A. J. Muste, leader of the American Workers Party; the Industrial

Workers of the World; the Communist party; Trotskyists from the Communist League of America; and radical intellectuals, among whom was Robert Lynd. It was also opposed by the NAACP and the Urban League as having little to offer black workers. In the post–World War II period, criticisms of the NLRA emerged from established labor leaders, including John L. Lewis in 1949 and the typographers in 1954 (Tomlins 1985, 313). In the more recent period large numbers of union leaders have attacked the NLRA, including Lane Kirkland, president of the AFL-CIO, C. Trost and L. M. Apcar, "AFL-CIO Chief Calls Labor Laws a 'Dead Letter.' " *Wall Street Journal,* 16 August 1984). Other union leadership statements against the NLRA may be found in U.S. Congress 1984. For contemporary academic criticisms of the NLRA as antilabor see Goldfield 1987; Lynd 1987; Rogers 1984; Tomlins 1985; and Weiler 1983, 1984.

5. Space considerations limit my consideration of other theories here.

6. For insightful discussions of these issues see Lakatos 1970, esp. 132–138, 154–77; Miller 1988; and Putnam 1978.

7. This category would include Lindblom's (1982) "structural veto of capital" and the ability to control the agenda emphasized by Bachrach and Baratz (1962).

8. Marxists, beginning with Marx, have often regarded certain reforms as a least partially beneficial for the working class. The successful struggle for the shorter workweek (discussed extensively in volume 1 of *Capital* [Marx 1962]), the abolition of child labor, legal rights for trade unions, and the full extension of the franchise have always been regarded as having much positive potential for the working class. Some Marxists, including Lenin and Gramsci, have stressed the contradictory nature of certain class reforms, both their empowering and socially integrative potential.

9. A timely example might be Corliss Lamont, the radical son of J. P. Morgan partner Thomas Lamont, in the 1930s and later; separating Corliss Lamont and his political activities from the "Morgan interests" does not require great acumen, nor does it require a denial that Thomas Lamont and others did act politically to represent those interests.

10. Block (1977) and to a certain extent Ferguson (1984) are exceptions.

11. The degree to which Roosevelt and New Deal Democrats were under siege by the capitalist class is suggested in the 1 April 1935 Senate labor committee testimony of Donald Comer, representing the Cotton Textile Institute, a strong opponent of the NLRA; Comer begins by asserting, "I think the present administration, the present Democratic Party, has given to the South the first and only opportunity of economic freedom we have had since the Civil War in its program of giving us parity prices for our cotton" (NLRB 1985, 2066), hardly the opening salvo for class warfare.

12. Skocpol, for instance, takes issue with this account (1980, 187). The accusation, discussed in the last section, that Finegold and Skocpol make about the impossibility of prolabor reforms under capitalism also seems to be an implicit targeting of this model.

13. I take the remarks by Finegold and Skocpol (1984, 189) to be using this model implicitly.

14. The state autonomist model, as presented by Skocpol (1980, 189) does limit the options to these choices.

15. Thus, one might say that those whose primary focus was on the negotiation process itself and the negotiators had chosen a unit of analysis too temporally and spatially circumscribed.

16. At most, 5%–8% of these newly organized workers participated in the NLRB procedures. Even growth under NLRB auspices, particularly during the 1930s, does not prove that it played a significant causal role. In some—perhaps most—cases an NLRB election was merely a face-saving device for an employer after workers had successfully won recognition through strikes or other means.

17. How this relates to states and parties is, of course, far from clear.

18. In the general glorification of John R. Commons and his followers and the correct recognition of their important innovative role in much social reform legislation, their corporate ties are rarely given due weight. As Domhoff (1970, 133; 1987, 171) notes, Commons was hardly influential before he went to work for the National Civic Federation in 1900. When he left in 1907, one-half of his academic salary at the University of Wisconsin was paid by two corporate leaders, a constraint making his characterization as an independent liberal reformer, at a minimum, open to questions.

19. See n. 3.

20. Various factors emphasized by other analysts are more difficult to separate from the protest movement than they appear at first sight. These factors often seem much more adequately described under the rubric of state-society interactions than as a result of autonomous "state and party" activity. Certainly the leftward shift in Congress as a result of the 1934 elections, the sympathetic response of NLRB officials to workers and their unions, and even the highly effective exposures of corporate repression by the LaFollette Committee are inextricably entwined with the strength and moral force of the mass working class movements of the early 1930s.

21. Delineating these points should satisfy the legitimate demand that one must specify the mechanisms by which labor influenced the reform process (Skocpol 1980, 185–86).

22. For other aspects of my hypothesis, I rely heavily on previous writers. This is particularly true for the investigation of corporate-reform ties. My

main goal here is merely to suggest the plausibility of an alternative hypothesis. The whole thesis will be discussed more fully in a longer, forthcoming work on the 1930s.

23. Roosevelt's secretary of Labor, Frances Perkins, notes the dominance of unemployment over all national political life in 1933 (Perkins 1946, 182–83).

24. Skocpol refers to work by Timothy Massad (1980), who tries to minimize the extent of activities of the unemployed by asserting that most protests after the 6 March 1930 demonstrations took place in three cities: New York, Chicago, and Detroit. This, however, seems unlikely. Virtually every activist biography and strike account gives evidence of numerically large, if oftentimes volatile and unstable, struggles and organizations of the unemployed during the early 1930s. The most comprehensive references (e.g., Rosenzweig 1976) list dozens of cities across the country, including several in the South, where interracial movements existed. The more likely assumption (in line with virtually all other accounts of protests of the unemployed during the early 1930s) is that activities of the unemployed—whether they were as persistent or as numerous as in New York City, Chicago, or Detroit or not, whether reported in the newspapers or not—were frequent, with roughly the same characteristics across the country.

25. Even those largely hostile to the role of radicals in the labor movement generally recognized these connections:

> Literally tens and maybe hundreds of thousands of American workers who might not otherwise ever have heard of radical political parties and programs, became sympathizers of these programs. Having found that the left-wing leadership of their unemployed organization fought militantly for their benefit on a day-to-day basis and in a great number of places and instances won real measures of success, workers certainly must have been less suspicious or even enthusiastic supporters, of left-wing leadership if they found it in local unions which they subsequently joined. (Karsh and Garman 1957, 96).

26. Out of hundreds of statements, I have yet to find one contemporary observer who echoes Skocpol.

27. Among their concerns were an opposition to the NLRA's principle of majority rule; most employers preferred proportional representation. They also opposed the banning of company unions and the limitation of unfair labor practice violations to employers.

28. See Rogers 1984 for a similar argument based on an analysis of the bill's numerous provisions.

29. Tarrow (1989), in his examination of Italian protest data, finds similar seasonal variations during cycles of protest in the 1967-73 period. Kennan (1986) argues that seasonal variations characterize strikes in *all* countries (pp. 1129–31).

30. As an example of this range consider the following: the ending of car production during World War II had an absolute impact, ending the purchase of new automobiles for the duration; the banning of alcohol production through prohibition, on the other hand, merely transferred its production to the illegal sphere, hardly diminishing the volume of consumption; women's suffrage, enacted with the Twentieth Amendment in 1920, was almost immediately put into effect; while the Fifteenth Amendment of the post–Civil War period, which mandated universal African-American suffrage, was not fully operational until almost a century later. One could go on with examples.

31. Many of the early standard writers, even though they argue that the New Deal played a central role in encouraging, stimulating, and giving birth to the labor upsurge, are willing to entertain the opposite view. Karsh and Garman, for instance, state,

> Moreover, the new unions were products of broad social forces in which countless individuals made important contributions. . . . One might even go further and argue, though the authors do not necessarily hold this view, that had the Board [NLRB] not been given a mandate to advance and protect union organization, and had the Board's personnel not been pro-labor, the militancy of the new unionists might have been more successfully organized by the left-wingers in the unions for more radical and wide-sweeping social change. (1957, 111)

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RESEARCH NOTE

GUNS VERSUS BUTTER: A DISAGGREGATED ANALYSIS

ALEX MINTZ
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Prior studies of the guns-versus-butter trade-off have focused on total military expenditures and subcomponents of welfare spending (education, health, and housing). I extend the analysis to include the major subcomponents of the defense budget. The results are consistent with Clayton's; Domke, Eichenberg, and Kelleher's; and Russett's findings of a lack of defense-welfare trade-off in the 1947–80 era but reveal very specific trade-offs during the Reagan years.

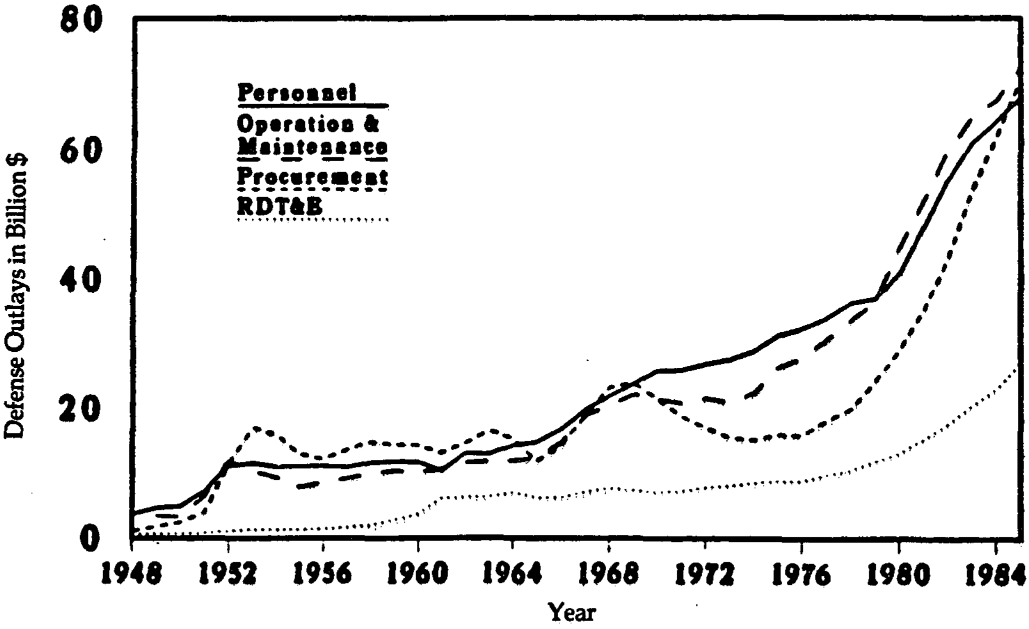
Most people believe there is a trade-off between spending on defense and spending on welfare (Russett 1982); but as a number of scholars (e.g., Clayton 1976; Domke, Eichenberg, and Kelleher 1983; Russett 1982) have shown,[1] the existence of such a trade-off is difficult to establish empirically. Prior analysis has centered on trade-offs between total defense spending and specific kinds of welfare spending (e.g., health, education, housing). I examine trade-offs between welfare spending and specific kinds of defense expenditures.

Congress appropriates funds to the Department of Defense (DoD) by budgetary component. The major DoD components are military personnel, military procurement, operation and maintenance (O&M), and research and development. These components typically account for 92–97% of the defense budget. Appropriations for military personnel finance "the personnel costs of the active duty forces of the United States and the future retirement benefits of the current active forces" (U.S. Office of Management and Budget 1987, *Budget,* p. IG1). These appropriations roughly index armed forces strength and account for about one-third of the defense budget. Procurement expenditures finance the acquisition of military equipment, weapons, and spare parts and the modification of existing equipment. Military procurement spending typically accounts for more than a quarter of the Department of Defense budget. Operation and maintenance appropriations finance the cost of repair and maintenance of DoD plant and equipment, fuel and supply costs, and remuneration of DoD civilian employees. Operation and maintenance expenditures account for about one-quarter of the budget. Research and development expenditures finance the development of strategic weapons systems, tactical programs, intelligence and communication systems and defense-oriented research. This subcategory accounts for almost 10% of the budget.[2]

DoD data (U.S. Department of Defense 1978, 1988) show that research and development expenditures were always the smallest of all major defense spending subcategories. Procurement spending was the largest subcategory between 1953 and 1964, between 1968 and 1969 and in the mid-1980s. Between 1948 and 1952 and between 1969 and the peak of the Reagan defense buildup in the mid-1980s military personnel and operation and maintenance expenditures were the largest (see Figure 1).

There is no reason to expect the impact

Figure 1. Fluctuations in U.S. Defense Spending, 1948–85



Source: Budget of the United States, 1948–85, Appendixes.

on welfare spending of allocations to the procurement of the B-1 bomber, the B-2 Stealth bomber, the Nimitz class carrier, the Trident submarine or the MX "Peacemaker" missile to be similar to the impact of spending for O&M programs or salaries and benefits of over two million active uniformed personnel. The Department of Defense is the largest single purchaser of goods and services in the national economy (DeGrasse, McGuinness, and Ragen 1983). It buys goods and services from about 300 thousand suppliers, and signs 52 thousand contract actions every working day (U.S. Department of Defense 1986). It also supports 2.1 million uniformed personnel (a quarter of whom are stationed overseas) and employs over 1 million DoD civilians, more than 1 million military reservists, and more than 2 million in the defense industry (U.S. Department of Defense 1988). DoD data show (U.S. Department of Defense 1983) that allocations of one billion dollars to military personnel programs support 49 thousand individuals while allocations of a similar amount to Defense nonpay programs create only 35 thousand jobs. One may expect that military procurement and R&D programs will also have an impact on welfare spending different from that of military personnel and vice-versa. The impact of each of the major defense subcategories on the major components of welfare spending is assessed below.

Research Strategy and Empirical Findings

Clayton (1976), Russett (1982) and Domke, Eichenberg, and Kelleher (1983) found no evidence for a defense-welfare trade-off in the post-1945 United States.

This seems to imply either that no subcategory of defense expenditure affects welfare spending or that subcategories of defense spending have positive and negative impacts essentially canceling one another out.

The major hypothesis I test is that there are variations in the impact of the different components of the defense budget on welfare spending. While allocations to DoD capital-intensive programs are hypothesized to take resources away from welfare programs, allocations to labor-intensive programs may ease the task of the government's health and education sectors rather than lead to a trade-off. Marfels (1978) has argued that the bulk of military expenditures for the procurement of military equipment goes to large, unionized "monopoly sector" firms. Galbraith (1967, 316) similarly points out that much of the military research and development budget goes to large, oligopolistic corporations that need R&D for "technical and scientific" advance. Huisken (1982, 8) agreed that military procurement expenditures are concentrated in the capital and technology-intensive sectors of industry:[3] labor in the defense industry is highly skilled and relatively well paid. A very low proportion of blue-collar workers and a very high proportion of engineers and scientists are involved in the development and production of weapon systems—a workforce hardly in need of welfare programs. Peroff and Podolak-Warren (1979, 24) have argued that the cost of weapon systems is therefore carried to a greater extent by lower-income groups who have greater need for welfare programs and "could use additional income for 'necessities' such as health services." Wildavsky (1988, 380) has similarly concluded that more for weapons came to be viewed as "an inegalitarian taking from the worst-off elements of the population." Given an upper limit to the federal budget, an increase in allocations to weapons systems could come at the expense of welfare spending.

Military personnel expenditures (and part of the O&M budget) directly go, in contrast, to finance the pay, allowances, clothing, subsistence, insurance, and retirement benefits of people, that is, they may alleviate the demand for health, education, and housing programs. Military personnel expenditures may help equalize the distribution of benefits, education, and medical care. In periods of high unemployment, increases in DoD personnel expenditures can even help middle- and lower-income groups, of which the armed forces largely consists. For some, military service is one of few ways they can not only contribute to national security but also acquire skills, job training, and technical expertise and gain experience not otherwise available, as is evident by the increasing overrepresentation of lower-income class and racial groups in the ground forces. Binkin and colleagues (1982) argued that the armed forces provide lower-income groups with their only chance to escape from the underclass. Military service became an extension of welfare policy for those who could not make it in the larger society.[4] Since the armed forces are providing health and education services to many who might otherwise be in need of government support, there may be a lesser need for the government to spend additional amounts of money on these programs. An increase in military personnel expenditures may therefore not necessarily come at the expense of welfare. Furthermore, personnel expenditures of the Department of Defense are largely "uncontrollable." Procurement and R&D spending are more "controllable" and much more volatile, however. Procurement spending, in particular, fluctuates considerably over time. It is expected that during periods of modernization of weapon systems, launching of new systems, and so on,[5] sharp increases in R&D and procurement expenditures will come at the expense of welfare spending.

To test for the accuracy or inaccuracy of this hypothesis, I replicated Russett's study (1982) of the guns-versus-butter trade-off while substituting total military expenditures by each of the four major subcomponents of the defense budget. Operational definitions and data on all variables (except the four components of the defense budget) were taken from Russett 1982. Data on military personnel expenditures, procurement spending, O&M expenditures and R&D expenditures were taken from the *Budget of the U.S. Government* 1947–87. The first year for which data are available on all variables is 1948 (1949 for the O&M subcategory). As Russett (1982, 776) points out, the Reagan presidency is different in its budgetary priorities. Since trade-offs during the Reagan years are evident (Kamlet, Mowery, and Su 1988; Wildavsky 1988), the inclusion of the Reagan years in my analysis would undoubtedly have distorted the findings. Following Russett, the Reagan buildup is analyzed separately. Russett's equations (1982, tbls. 2, 3) are

$$\begin{aligned}\%\Delta\,\text{education} = {} & a + b1\%\Delta\,\text{housing}\\ & + b2\%\Delta\,\text{health} - b3\%\Delta\,\text{military}\\ & + b4\%\Delta\,\text{taxes}\\ & + b5\%\Delta\,\text{enrollment}\ (t-1)\\ & - b6\text{Republican}\ (t-1)\\ & + b7\%\Delta\,\text{GNP}\ (t-1) + e\end{aligned}$$

$$\begin{aligned}\%\Delta\,\text{health} = {} & a + b1\%\Delta\,\text{housing}\\ & + b2\%\Delta\,\text{education}\\ & - b3\%\Delta\,\text{military} + b4\%\Delta\,\text{taxes}\\ & + b5\%\Delta\,\text{population-over-65}\ (t-1)\\ & - b6\text{Republican}\ (t-1)\\ & + b7\%\Delta\,\text{GNP}\ (t-1)\\ & + b8\text{medicare}\ (t-1) + e\end{aligned}$$

Russett has also estimated two reduced equations, excluding %Δhousing and %Δ health from the education equation and %Δ housing and %Δ education from the health equation. Data are in constant prices and represent final outlays (see Russett 1982, tables 2, 3). As Domke, Eichenberg, and Kelleher (1983, 21) explained, it is indeed "best to investigate final outlays—the bottom line of the ledger comprising the many alterations of the budget and its final implementation." In any case data on the components of the defense budget in all other stages of the budgeting process were not found for the entire time period.

The results of the disaggregated analysis are shown in Table 1. Column 1 in this table reproduces the results of Russett 1982. Column 2 reports my replication. Columns 3–6 show estimates of Russett's equation, but each time with a different component of the defense budget serving as an independent variable. The analysis of total military spending produced results very similar to Russett's and consistent with Domke, Eichenberg, and Kelleher's (1983). More importantly, the results of the disaggregated analysis have likewise shown a lack of defense-welfare trade-offs. The coefficients for military personnel, procurement, O&M and R&D in the education equation are all nonsignificant at the .05 level (see Table 1). Estimates of the reduced equations, generalized least squares corrections for autocorrelation and the inclusion of additional controls such as battle deaths and population under 18, have produced insignificant results for all four components.[6] Zero-order correlations between education and procurement (.22) and education and R&D expenditures (.09) are likewise nonsignificant. The correlations between personnel and education expenditures and O&M and education spending are a bit higher (.34 and .35 respectively) but still nonsignificant at the .01 level. Furthermore, estimates of Russett's health equations (not shown here) produced results very similar to the education equations,

Table 1. The Impact of Defense Spending on Education Expenditures

Variable	Total Military Expenditures[a]	Total Military Expenditures	Military Personnel	Procurement	Operation and Maintenance	Research, Development, Testing, and Evaluation
Constant	.097** (.048)	.155 (.123)	.079 (.112)	.066 (.120)	.158 (.114)	.142 (.114)
%Δ housing	.014* (.004)	.016* (.005)	.018* (.005)	.019* (.005)	.017* (.005)	.017* (.005)
%Δ health	.179** (.102)	.232** (.107)	.230** (.100)	.252** (.102)	.235** (.107)	.231** (.106)
%Δ military	.086 (.190)	.120 (.283)	.480 (.252)	.145 (.085)	.296 (.197)	.127 (.146)
%Δ taxes	.508 (.562)	.331 (.805)	.321 (.429)	.415 (.422)	-.067 (.645)	.544 (.432)
%Δ enrollment ($t-1$)	.982 (.919)	-.000 (.000)	-.000 (.000)	-.000 (.000)	-.000 (.000)	-.000 (.000)
Republican ($t-1$)	-.185 (.415)	.007 (.050)	.047 (.051)	.036 (.050)	.005 (.049)	-.007 (.049)
%Δ GNP ($t-1$)	-.001 (.007)	-.006 (.011)	-.008 (.010)	-.009 (.010)	-.009 (.010)	-.007 (.010)
Adjusted R^2	.35	.32	.41	.39	.37	.34
Durbin-Watson	1.62	1.64	1.58	1.66	1.53	1.62

Note: Calculated based on data obtained from Russett 1982 and the *Department of Defense Annual Report.*

[a] Russett 1982, 772

*$p \leq .01$.

**$p \leq .05$.

indicating (again) a lack of defense-welfare trade-offs. Zero-order correlations of health and personnel, procurement, O&M, and R&D range from −.05 to .14 and are likewise nonsignificant.

It appears therefore, that the growth in military personnel expenditures in 1947–80 has not been large enough to substitute for, or reduce the demand for, welfare programs and create a defense-welfare trade-off. On the other hand, the rise in procurement expenditures has not "solved" or alleviated the demand for health and education programs, which have continued to grow independently of the growth in procurement spending. And the magnitude of the increases in procurement and R&D spending has not been large enough to shift resources from one budgetary component to the other in an expanding budget. The purpose of Russett's studies (1969, 1982) was to test for systematic trade-offs between military spending and welfare spending. My disaggregation of the analysis of U.S. total military spending into analysis of major subcategories of military spending has likewise revealed no such trade-offs.

Recent Trade-offs

Korb (1987, 166) has pointed out that "the FY1985 defense budget was more than double that of the FY1980 budget, the last full budget of the Carter Administration. From FY1980 to FY1985 the defense budget grew from $142.6 billion to $286.8 billion." Even in constant prices, the increase over that time period

Table 2. Percentage Change in Defense, Education, and Health Expenditures, 1981-87 (Constant 1982 Prices)

Year	Military Personnel	Procurement	Operation and Maintenance	Research, Development, Testing, and Evaluation	Education	Health
1981	5.9	9.6	4.7	5.2	-3.0	6.2
1982	7.4	14.7	7.4	8.3	-24.4	-3.7
1983	7.0	20.2	5.5	12.4	-2.9	2.9
1984	1.7	11.4	.2	8.6	-.4	2.1
1985	3.4	11.2	5.0	14.7	3.8	7.6
1986	3.9	7.1	2.5	17.4	4.2	7.1
1987	-2.7	6.3	.2	4.1	-9.6	3.5
Average	3.8	11.5	3.6	10.1	-4.6	3.7

Source: The *Department of Defense Annual Report*; and *Historical Tables: Budget of the U.S. Government.*

was in excess of 50%. During the Carter years, military expenditures accounted for 22-23% of total federal outlays and 4.8 to 5.0% of the GNP. During the Reagan years, they rose to about 25%-29% of total outlays and 6%-6.6% of the GNP. Both Russett (1982, 776) and Wildavsky (1988, 380) pointed out that the buildup has resulted in trade-offs between military spending and civilian spending. Kamlet, Mowery, and Su (1988, 1304) have shown that defense spending was higher and controllable domestic spending substantially lower under Reagan "than would have been the case under pre-Reagan budgetary priorities."[7] As Wildavsky (1988) pointed out, "domestic policy became defense policy in that slimming the former became the way to fatten the latter. Defense policy became domestic policy in that more for defense became less for domestic, mostly welfare programs. . . . The Reagan administration sought to get [for defense] as much as it could as fast as it could for as long as it could."

I examine specific trade-offs between welfare spending and (1) military personnel expenditures, (2) O&M outlays, (3) procurement spending, and (4) R&D expenditures. The results (Table 2) clearly show that the trade-off in the 1980s is between the development and procurement of military systems and investments in education. To be more specific, procurement and R&D expenditures grew every year during that period by an average of more than 10%, while education expenditures were cut (in constant prices) in five out of the seven years on an average of almost 5% (1.3% if 1982 is excluded). Military personnel and O&M outlays rose, on average, by only 3.8% and 3.6% respectively. Health expenditures grew by 3.7% annually (see Table 2).

One explanation for the weapon systems. About 20% of the total DoD administration has not only raised military expenditures substantially but also changed priorities in the Department of Defense by putting much more emphasis on the purchase of equipment and material (i.e., the capital- and technology-intensive procurement and R&D programs) than on the so-called operating programs (i.e., the labor-intensive military personnel and O&M programs; see Table 3). More than 55% of the growth in military spending between FY1982 and FY1986 went to procurement of weapon systems and the development of new systems. About 20% of the total defense

Table 3. Resource Allocation in the Department of Defense: The Reagan Years versus the Carter Years (Average %)

		The Reagan Years	
Component	The Carter Years		1985–86 Only
Military personnel	32.5	28.8	27.2
Operation and maintenance	33.2	30.6	28.9
Procurement	21.7	27.1	28.8
Research, development, testing, and evaluation	10.0	10.7	11.6

Source: Budget of the U.S. Government (various years).

budget increase went to O&M programs, while less than 20% of the total growth has been allocated to the military personnel subcategory of the budget (see U.S. Office of Management and Budget, *Historical Tables*). Consequently, procurement outlays now account for almost 29% of the DoD budget, compared with less than 20% a decade ago. Research and development spending accounts for more than 12% of the budget compared with only 9%–10% a decade ago. Operation and maintenance expenditures account for 27%–28% compared with about 32%–33% in the mid-1970s, while military personnel outlays account for only slightly more than 27% of the defense budget in recent years, compared with 34%–35% a decade ago (U.S. Office of Management and Budget, *Historical Tables*). Despite claims by administration officials (U.S. Department of Defense 1988, 129) that "the recent defense buildup was not funded at the expense of vital domestic programs," there is a clear evidence that the buildup has resulted in cuts in education expenditures for which the administration has, evidently, less commitment than for health programs.

Conclusions

I have examined the guns-welfare trade-off at a less-aggregated level than has been done previously. The results are consistent with Domke, Eichenberg, and Kelleher's (1983), Russett's (1982) and Clayton's (1976) findings of no evidence of defense-welfare trade-offs in the pre-Reagan era. Although there may be years in which R&D or procurement spending increased and welfare spending decreased, the pattern is not common enough to indicate a trade-off between these spending categories over the entire period. As Domke, Eichenberg, and Kelleher (1983, 33) point out, "defense and welfare expenditures appear . . . to be driven by separate sets of determinants which do not require one to be systematically sacrificed for the other." These conclusions are reinforced by the disaggregated analysis. The rising costs of many welfare programs (e.g., medicare) and the "uncontrollable" nature of many of these programs might be important determinants. Another explanation might be the political "acceptability" of deficit spending until the mid-1980s.

It should be pointed out that my analysis is concerned only with the direct budgetary trade-off between guns and butter. It might be, however, that the indirect, second- or third-order (multiplier) effect of increased military spending—once played out through the whole economy—is actually negative.[8] Deger (1986, 190) has noted that as defense spending increases, it influences inflation, taxation, forced saving, and various programs and will therefore reduce the potential increase in welfare spending. Furthermore, the level of military expenditures as a proportion of the GNP is already high in the United States by West European standards (Ward 1984), while the level of social welfare spending, compared to most European countries, was moderate in the 1950s and 1960s (Clayton 1976, 378–79). More research on the direct effects of increased defense spending on welfare spending is therefore needed.

As for the Reagan years, the analysis reveals the existence of trade-offs between investments in the development and production of weapons systems and spending on education. Since during the six years Republicans controlled the Senate, Secretary Weinberger's defense requests were reduced by an average of 7%—more than for any previous secretary of Defense (see Korb 1987, 168)—and since Reagan's requests for additional cuts in welfare spending were not accepted fully by Congress, the trade-off could have even been more severe.

Kamlet, Mowery, and Su (1988, 1304) showed that the Reagan administration departed dramatically from the "broad budgetary priorities" of other administrations. I have shown that the defense-welfare trade-off in the 1980s is specifically between military investments in weapons systems and investments in human capital (and not between military personnel and education, O&M and health, or military personnel and health). Since priority is now being given to deficit reduction, it is difficult to imagine drastic increases in allocations to both weapons systems and welfare expenditures, especially since the trade-off during the Reagan years was evident in an expanding budget with a huge deficit. The Bush administration will have to decide on whether to continue the present trend, to reverse the trend, or to allocate less to both guns and butter.

Notes

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1. For a comprehensive review of the literature on this topic see Chan 1985.

2. Other components of the Department of Defense budget are military construction, family housing, and allowances.

3. Huisken (1982) points out that military industrial activity can be viewed as labor-intensive only at the stage of final assembly of weapon systems.

4. According to Berryman's data, however, the military *cannot* be viewed as an "employer of last resort" (1988, 4).

5. One may argue that the cost of the government's bail-out of defense industries (the so-called "welfare capitalism") also comes at the expense of other programs.

6. SUR estimates have likewise produced insignificant results.

7. Kamlet, Mowery, and Su (1988) found, however, that *uncontrollable* domestic spending was higher under Reagan than under his predecessors.

8. Deger (1986) showed that while the first order effects of military spending on growth in less-developed countries are often positive, the multiplier effects are clearly negative. Mintz and Ward (1989) showed that military spending and corporate profits are typically interrelated. Smith (1980) found that while an increase in defense expenditures does not significantly affect the social wage in developed countries, the burden falls on other programs (e.g., investment).

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CONTROVERSIES

HOW MUCH DO INTEREST GROUPS INFLUENCE STATE ECONOMIC GROWTH?

In the March 1988 issue of this Review, *Virginia Gray and David Lowery presented a respecification of Mancur Olson's model of economic growth and tested the revised model with U.S. state data. A special feature of the Gray-Lowery analysis is its more thorough measurement of interest group effects. These investigators found interest group influences quite different from those anticipated by Olson's model. Paul Brace and Youssef Cohen argue that the Gray-Lowery model misspecifies the determinants of state economic growth, overstating the role of interest group size and failing to incorporate crucial exogenous variables. Gray and Lowery join the issue and defend their specification.*

In *The Rise and Decline of Nations* Mancur Olson (1982) developed a powerful theory he claimed could explain, among other things, differences in the growth rates of the states. Since the book's publication, most of the attempts to test Olson's hypothesis have sought to improve on earlier tests (which assessed the effects of interest groups only indirectly) by collecting relevant data on interest groups in the states to measure their impact on the rate of growth directly.[1] Perhaps the most sophisticated effort along these lines is the recent paper by Virginia Gray and David Lowery (1988). With this article Gray and Lowery have given us an ingenious, well-crafted test involving a laborious and painstaking collection of data on interest groups in the states. It is with some genuine reluctance, therefore, that we take issue with the Gray-Lowery thesis. We believe that the hypothesis on interest groups and economic growth they tested is not that implied by Olson's theory. The Gray-Lowery test is based on the critical interpretation of Olson's argument developed by James W. Dean (1983), an interpretation largely incongruent with the gist of Olson's explanation of differences in the growth rates of the states.

An additional problem with the Gray-Lowery test is its failure to consider important exogenous determinants of state growth. The performance of the national economy, as well as the international economic situation, can have a substantial impact on the growth of the states. It follows that we must control for these exogenous variables to assess the effect of special interest groups on state economic growth accurately. Even if Gray and Lowery had tested the correct hypothesis, they fail to take exogenous variables at the national and international level into account. In all fairness, Gray and Lowery are not alone in failing to incorporate these exogenous factors. Yet precisely because such important factors have been so widely neglected, we shall demonstrate that their inclusion could generate very different results concerning the effect of interest groups on economic growth.

Olson's Hypothesis

Building on his work on the logic of collective action, Olson argued that when it comes to working for the common good, special interest groups that are small in relation to the economy act analogously to individuals in large groups. Just as rational individuals in large groups have no incentive to work spontaneously for the common interest of their group, relatively small interest groups have no incentive to work spontaneously for the common interest of their society. Both the individual in a large group and the relatively small interest group would have to bear costs far in excess of benefits if they acted in the common interest of the larger group to which they belong. Consequently, special interest groups prefer to act in their narrow self-interest to the detriment of the common interest of society.

Olson showed the importance of this argument to the study of economic growth. He reasoned that although economic efficiency and growth are in the common interest of all groups in society, each relatively small special interest group has no incentive to work spontaneously for an increase in efficiency and output. Since any such group is minute relative to the economy as a whole, its members would get only a miniscule share of the benefits of their effort to improve output, whereas they would have to bear all the costs of that effort. Small groups thus prefer increasing their slice of the pie to increasing the whole social pie. These groups will fight for higher tariffs, price supports, tax advantages, and higher wages, all of which prevent an efficient allocation of resources. In addition, narrow interest groups tend to block innovation. Firms will use their trade association or cartel to block the innovations of their competitors, and unions will attempt to repress labor-saving innovations that would reduce the demand for the groups they represent.

Not surprisingly, Olson concluded that other things being constant, the higher the number of special interest groups in a society, the lower its rate of economic growth. Applied to the states his hypothesis, then, is that the higher the number of interest groups in a state, the lower its rate of growth.

Dean's Hypothesis

James W. Dean (1983, 249) criticized Olson for failing to recognize the political importance of business groups. According to Dean, Olson focused on "antigrowth" groups such as labor unions, farm organizations, and professional associations, without paying much attention to the countervailing power of "progrowth" business groups. Dean argued that these groups cannot be neglected because their political power is sufficient to block the efficiency-reducing activities of antigrowth groups. If so, Olson is wrong, because the net effect of interest groups on economic growth would no longer be negative.

Dean argued that the rate of economic growth depends on the balance of power between progrowth business groups and antigrowth groups such as unions and environmentalists. His hypothesis, then, is that (other things being the same) the greater the power of business groups relative to that of antigrowth groups, the higher the rate of economic growth (Dean 1983, 254).

This may be an interesting hypothesis, but it is completely antithetical to Olson's argument. Olson gives much attention to the power and the effect of business groups. His texts (Olson 1982, 41–47; 1983, 16–24) contain telling examples of firms and business associations acting in ways that reduce efficiency and economic growth. He mentions firms, cartels, and trade associations pursuing their narrow self-interest through oligopolistic arrangements, tariffs, price supports, and tax loopholes—not to speak of firms that "have an incentive to use their trade association or cartel, either directly or indirectly or through its links with the government, to obstruct the innovation [of a competitor]" (Olson 1983, 19).

It is not that business groups are against efficiency and growth; anyone would concede that business groups are "progrowth." As a matter of fact, Olson (1982, 41–42) begins his discussion about the effects of interest groups by pointing out that they want growth:

> Normally all such organizations, whatever their scale or form, have reason to want economic efficiency and growth, and good fortune generally, for the society in which they operate. Whatever type of goods or labor the members of an organization sell, normally the demand for it will be greater the more prosperous the society. . . . Similarly, the available technology will generally be better and the goods (though not the labor) that the members of the organization buy will generally be cheaper if they live in a more productive society.

For Olson, both business and labor groups are progrowth in the sense that they want efficiency and economic growth. Yet these groups, including business ones, have no incentive to promote growth. The logic of collective action compels each of them to secure a greater share of the pie for its members; and in so doing, interest groups, including business groups, will reduce efficiency and decrease the economic output of society. Whereas both business groups and unions are progrowth in principle, they often are antigrowth in action. From the point of view of Olson's theory, therefore, it makes no sense whatsoever to say that the greater the power of business groups, the higher the rate of growth. If Olson is right there is no reason to expect business groups to damage the economy less than the groups Dean labels as "antigrowth."

The Gray-Lowery Test

There is no doubt that Gray and Lowery tested Dean's, rather than Olson's, hypothesis. They accepted Dean's characterization of business groups as progrowth and labor unions as antigrowth and proceeded to assess the effects of both the absolute and relative power of these groups on the rate of economic growth (Gray and Lowery 1988, 113). They first measured the impact of the absolute power of unions and their power relative to business and to government—in 1975—on the rate of growth of the states for 1977–82. They then measured the impact of the change in each of the independent variables for 1975–80 on the rate of growth for 1977–82. Finally, Gray and Lowery repeated the whole exercise for business groups, that is, they measured the effect of the absolute power of business and its power relative to unions and government in 1975 and for 1975–80 on the rate of state growth for 1977–82.

Although Gray and Lowery were clearly testing Dean's balance-of-power argument, they were not entirely off the mark on Olson's views about the role of government. Olson, in fact, has suggested (with some trepidation) that government can offset the negative effects of special interest groups (Olson 1983, 23–24). Since it is the most encompassing group, government has a strong incentive to protect the economy from narrow self-interested groups. Of course, its capacity to do so depends on its power relative to these groups. Therefore, in assessing the effect of the relative power of government, Gray and Lowery seemed to be testing Olson's views. They failed to test his views, however, because they accepted Dean's basic division between pro- and antigrowth groups. Whereas a test of Olson's argument requires measuring the effect of government's power relative to that of *all* groups, Gray and Lowery measured the effect of government's power relative to unions separately from that of its power relative to business.

The results of the Gray-Lowery test were largely negative (Gray and Lowery 1988, 127–29). Their independent variables had almost no effect on the rate of growth; and when they did have an effect, it was in the wrong direction. Since Gray and Lowery presented their work as testing Olson's hypothesis, their results could seem to show that something was seriously wrong with his theory. However, Gray and Lowery tested a hypothesis that is neither implied in, nor compatible with, Olson's theory. At best, what they did was reject Dean's hypothesis.

Even as a test of Dean's hypothesis, however, the Gray-Lowery test should be regarded with caution. Their measure of the power of a group is based on its size (Gray and Lowery 1988, 115). We need not dwell here on the problems of resource-based measures of power. It is sufficient to note that size is obviously not the major resource of business groups. As Dean (1983, 249) himself puts it, "Size of membership is far from a good proxy for such power, especially in the case of interest groups that represent business." It should be added that size is particularly misleading as an indicator of business power when comparing it to the power of unions, since unions can be formidably large.

Exogenous Variables

There is reason to believe that Gray and Lowery should have controlled for at least one exogenous variable when they assessed the effects of interest groups on the rate of state economic growth. The period they chose for their test, 1977–82, was a period of energy crisis; and the oil shock had a substantial effect on the economies of the states (Brace and Baumann 1988; Dye 1980; Miernyk 1977; Schwarz 1983). Rising prices of oil and energy-related resources greatly favored energy-producing states, thereby generating substantial differences between the growth rates of these states and those of energy-dependent states. If so, differences in these growth rates during the period studied by Gray and Lowery may be largely due to differences in their capacity to produce oil and energy-related resources. By omitting these energy differences from their analysis, then, Gray and Lowery ran the risk of falsely attributing differences in the performance of state economies to interest groups. To judge from recent empirical studies, this risk is very high; for they clearly show that the inclusion of variables measuring energy advantages and disadvantages can alter our conclusions concerning the effects of endogenous variables on economic performance dramatically (Lange and Garrett 1985, 1987; Jackman 1987).

The performance of states is also influenced by national market and policy forces beyond their control. To give only one example, patterns of federal expenditure may have a considerable impact on the behavior of state economies (Anton 1983; Leontief et al. 1965; Russett 1970). Much of the research done on this topic suggests that the federal budget has a redistributive bias: it redistributes wealth away from high-income states (Peltzman 1985, 656). The consequent equalization may in turn stimulate some state economies and retard others.

Obviously, we could offer other examples of exogenous variables likely to have a strong effect on state economies. But we have no intention of providing an exhaustive list of such variables. Rather than listing them, we want to show that the influence of exogenous variables as a whole is far too large to be omitted from tests involving the relationship between interest groups and state economic growth. To assess the weight of exogenous variables (as a whole) relative to that of endogenous factors we pooled time series of growth rates of the states and then measured the effect of exogenous factors by means of dummy variables for our time points and the effect of endogenous ones by dummies for the states.[2] The relative explanatory weight of each set of factors was determined by the proportion of the variance in the dependent variable explained by each.

Since we are dealing with the Gray-Lowery test, we used time series for the period they investigated, the six years 1977–82. We then pooled the series of annual growth rates over those years for each of the 48 states for which we had data and estimated the parameters of the following model:[3]

$$Y_{it} = \alpha + \Sigma\beta_i x_{it,i} + \Sigma\delta_t z_{it,t} + u_i$$

where $X_{it,i} = 1$ for the ith state, 0 otherwise ($i = 2, 3, \ldots, N$); $z_{it,t} = 1$ for the tth year, 0 otherwise ($t = 2, 3, \ldots, T$); and Y_{it} is the indicator of economic performance, which is measured in two different ways: annual growth of per capita personal income (1967 dollars) and of value added by manufacturing (1967 dollars). Since the *x*s capture the variation in overall economic performance due to the states and the *z*s the variation over time common to all states, the *x*s as a whole can be interpreted as capturing the variation due to factors endogenous to the states and the *z*s that due to exogenous factors outside their control.

The results speak for themselves. The *x*s explain 7% of the variance in the growth of per capita personal income and 13% of that in the growth of value added by manufacturing, whereas the *z*s explain, respectively, 47% and 39% of the variance. In other words, during the period investigated by Gray and Lowery, the influence of variables exogenous to the states is at least three times larger than that of endogenous factors (like those involving the behavior of interest groups).

Conclusions

We began our evaluation by showing that Dean's hypothesis is incompatible with Olson's theory. The hypothesis implied by that theory does not permit Dean's division of interest groups into pro- and antigrowth groups. It requires us, rather, to treat interest groups as a whole; for its prediction refers to their aggregate effect on economic efficiency and growth.

As for Olson's conjecture about the role of government, we again argued that the appropriate test would involve assessing the effect of the power of government relative to the aggregate power of *all* interest groups.[4] Short of having better measures of power than number and size, we may have to limit ourselves to assessing the effect of the absolute, rather than the relative, power of government to curtail the damaging behavior of interest groups. Any results based on indicators of the power of groups constructed with number and size are suspect.

We ended our evaluation by showing that variables exogenous to the states have too powerful an influence to be omitted from the study of the relationship between interest groups and economic growth in the states. Our empirical analysis leaves little doubt as to the necessity to pay far greater attention to exogenous variables.

We do not mean to suggest that only models involving the pooling of cross section and time series data allow us to test hypotheses like Olson's. We are well aware of the impossible demands such pooling would require. Collecting time series of data on interest groups is far beyond the means of most of us. All we want to suggest is that more of an effort should be made to identify major exogenous variables and to control for them in the empirical analysis of the impact of endogenous factors on the economic performance of states.

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Paul Brace and Youssef Cohen raise two fundamental issues about our test of Olson's model of economic growth, issues that are important both because of the widespread interest in Olson's model and the difficulties encountered by all who have attempted to confront it with data. Moreover, the implications of one of these issues go beyond the specific case of the Olson construct to raise questions about the intellectual status and appropriate interpretation of certain kinds of empirical tests used in the discipline. Indeed, at their core, the Brace and Cohen comments touch on such basic epistemological concerns as the meaning of falsification and the proper relationship between theory and data, concerns that reach far beyond our initial findings. Given this broader importance, we will use this opportunity to consider not only Brace and Cohen's specific criticisms of our test but also the more general issue of "tests of falsification." We consider their two major criticisms in turn, reveal the underlying contradiction between them, and discuss the inherent trade-offs made in conducting empirical tests of theories like Olson's.

First, however, it should be noted that Brace and Cohen question only one part of our empirical assessment of Olson's model. We assessed the relationships both between time and a number of interest group characteristics thought to be associated with economic growth and between those group characteristics and several measures of economic growth. Both of the issues raised by Brace and Cohen are limited to the specification of only the latter set of tests. It could be argued, however, that without some demonstrated empirical linkage between time and the interest group characteristics hypothesized to be associated with diminished economic performance, any resulting model, albeit useful, would not be Olson's. Thus, the remaining discussion takes us beyond the specific issue of whether Olson's model has some empirical foundation. As long as our findings on the relationship between time and interest group characteristics remain, our initial conclusions about the status of Olson's model stand.

Whose Hypothesis?

The first issue that Brace and Cohen raise suggests that our model is misspecified through *inclusion of an irrelevant explanatory variable* and that, therefore, we have failed to test Olson's model. Developing a better test of that model obviously was our central concern. Apart from essentially anecdotal comments on several nations, the only empirical support for Olson's model—Choi's analysis of economic growth in the American states (1979; see Olson 1983, 34–44)—was, we believed, based on an excessively simplified interpretation of Olson's theory and on rudimentary measures of its key concepts. Accordingly, the first part of our paper was devoted to developing a more theoretically satisfying specification of Olson's model that could then be tested with state data on economic growth. Brace and Cohen have little apparent trouble with most of this analysis. Rather, they restrict their first criticism to one part of our alternative specification: the interpretation of our measure of the relative power of business and labor groups.

Brace and Cohen argue that we have substituted Dean's (1983, 249) analysis of interest group conflict for Olson's because Olson says little about business-labor conflict. On narrow terms, this is certainly a valid criticism. Indeed, the central point of Dean's analysis, as well as that of Whitely (1983), was that Olson failed to consider such conflict. On broader terms, however, we believe that our specification is appropriate as a specific operationalization of Olson's interpretation of the role of interest group conflict in engendering or dampening growth. Clearly, Olson does consider the *relative* balance between growth supportive interests and growth retarding interests in his contrast of encompassing and nonencompassing groups. In terms of specific applications of this concern for relative power, Olson notes (1983, 23) that his argument "focuses not only on the absolute power of special-interest groups but also on the *relative* strength of the most encompassing group, the government, versus constituent groups that are less encompassing." It is this underlying conflict, which is fully and deeply embedded in Olson's analysis, that we were trying to tap in our measures of the relative power of union and business interest groups. What is at issue, then, is not so much a misinterpretation of the part of Olson's theory dealing with the relative power or pro- and antigrowth interest groups as the validity of our specific operationalization of this component of his model. Framed thus, the issue raised by Brace and Cohen is less challenging of our analysis, but nonetheless important.

Even if framed as a narrower issue of operationalization of a concept found in Olson's model, we still have to consider whether this operationalization is fair. We believe it is fair, though partial and incomplete. As Dean and others have pointed out, labor-business conflict is fundamental to our economic system and might, therefore, be expected to reflect some of the underlying tension between growth and antigrowth forces. Further, our balance-of-power operationalization is an especially realistic one for the state governmental context; it is well documented (Hrebenar and Thomas 1989; Zeigler 1983) that business is the most powerful organized interest in most states and that labor's strength is declining. Therefore, to focus just on labor unions—as Olson did and as Brace and Cohen seem to imply that we should—would overrate labor's influence in state capitals. Still, we would be the first to admit (and did admit) that this operationalization is partial and incomplete. These are not the only competing forces in our interest group system that might reflect progrowth and antigrowth interests. Empirical consideration of other dimensions of potential interest group conflict reflective of this underlying balance would be useful.

Before we look at alternative dimensions of interest group conflict, however, we must further consider whether our specific indicators of labor and business group power can reflect the underlying issue of pro- versus antigrowth sentiments. Brace and Cohen seem to imply that our measures fail to distinguish between progrowth and antigrowth interests of both business and labor, noting that while "both business groups and unions are progrowth in principle, they often are antigrowth in action." We fully agree, and this was just what our indicators were designed to measure. And contrary to the assertion that our "measure of the power of a group is based on its size," we do not rely on measures of group size alone. Size in our analysis usually refers to the number of groups registered to lobby the legislature. Importantly, these measures were then combined with other indicators of group power. The group power measures were complex interaction terms that included component indicators designed to tap forces that would tend to make a given sector of economic interest representation more or less supportive of growth (the number and degree of encompassingness of interest groups as well as their size). Thus, the group power terms of the model actually *represent power vis-à-vis an antigrowth orientation*, as opposed to influence per se. On this issue we believe that Brace and Cohen are simply missing the underlying structure of the indicators we employed and ignoring the fact that Choi and Olson's analysis was based exclusively on measures of interest group size—labor union membership.

Still, other dimensions of interest group conflict might better reflect the underlying conflict between pro- and antigrowth forces than the labor-versus-business dimension that we considered, irrespective of the complexity built into our measures. One important possibility is the conflict between industrial and postindustrial business interests. On its face, it would seem plausible to expect that the latter would tend to harbor antigrowth sentiments while the latter—comprising a younger and more vigorous set of businesses—would tend to be more supportive of growth. We considered just this dimension of interest group conflict in our original version of the paper (Gray and Lowery 1986); and we tested models for each of the dependent measures using industrial group power and the relative power of industrial groups to postindustrial groups and to government as independent variables in the same form as those that appeared in the final publication of the paper for labor and business groups. Because the findings for these analyses were essentially the same as those for labor and business groups in terms of the weak support they provided for Olson's model, they were dropped from the final paper in the interest of space. This does not mean that we believe there is no dimension of interest group conflict better reflective of Olson's conception of the relative power or pro- and antigrowth interests than the two we considered, only that neither of these two provides much support for Olson's model. We remain open to suggestions on other dimensions.

Finally, we might consider what impact Brace and Cohen's argument has for the interpretation of our empirical results. Even if we accept their argument that the relative business and labor power variables were inappropriately included in the model, our negative findings by and large stand unopposed. Brace and Cohen do not challenge the business power, union power, and power-relative-to-government terms of our models. At worst, then, our use of the relative business-labor power terms in the models represents, from their point of view, inclusion of irrelevant explanatory variables. Inclusion of irrelevant explanatory variables do no great damage to empirical estimates. Barring the possibility of increased collinearity, which we discussed fully in the original paper, the estimates for the remaining terms in the model are still unbiased. And the incorrect signs of the estimates for the variables that Brace and Cohen agree *should* be in the model represent the most severe challenge to Olson's model in our results.

Exogenous or Extraneous: The Role of Falsification

The second criticism Brace and Cohen raise concerns misspecification through *exclusion of relevant explanatory variables.* In a narrow sense, they are again correct. There are any number of other factors that almost certainly influence the pace of economic growth, indeed, many more than Brace and Cohen suggest. We fully admitted as much in the conclusion of our paper (Gray and Lowery 1988, 128–29) when we claimed that our test, like Choi's, was far too simple to capture all that influenced economic growth rates in the states. But the key issue here is not whether we included all of the necessary explanatory variables to account for economic growth but whether the empirical results met the specific purpose of our test.

Our central argument was that previous tests (i.e., Choi 1979; Olson 1983, 34–44) represent Olson's model as a monocausal relationship and assessed that simple relationship using inadequate measures of several important interest group concepts. Instead, we suggested that the underlying relationships between time, interest group characteristics, and economic growth suggested by Olson implied a much more complex sequential chain of causal relationships, which we then proceeded to decompose while developing adequate measures of the central concepts therein. Our purpose was quite limited, however. We were simply trying to develop a better test of Olson's model as presented by Choi. We were not trying to extend Olson's model beyond the framework in which he presented it by developing a comprehensive analysis of economic growth. In an important sense, then, our analysis was designed to *falsify* Choi's work and Olson's interpretation of it. Within that limited purpose, we did not believe it was necessary to consider more than Olson and Choi had considered. From this admittedly limited perspective, Brace and Cohen's criticism is correct but also irrelevant.

Equally irrelevant is their empirical exercise with pooled data. Choi's test and Olson's interpretation of it analyzes change in growth rates as a function of time since statehood, which they consider a proxy for cross-sectional variations in interest group structure. Olson and Choi simply do not consider the kinds of temporal influences that Brace and Cohen assert are so important, nor do they use a pooled design. By employing change scores, we intentionally controlled for the influence of the kinds of temporal forces discussed by Brace and Cohen in order to focus exclusively on Olson and Choi's claims. It is true that by "controlling" for those other temporal variables we were excluding from the analysis some of the most important determinants of economic growth. But that was our goal. We wanted to evaluate Olson's evidence using Olson's own concepts and modes of analysis, not develop a non-Olsonian model of economic growth.

This interpretation of our purpose and our results, while it does serve to sidestep the second issue raised by Brace and Cohen, also raises a very important issue that goes beyond our paper. That is, What is the epistemological status of such falsification exercises? We have all come a long way from the days of simple Popperian falsification. More popular today is Lakatos' (1978) view that no simple exercise in falsification can by itself challenge an ongoing research program; only a new research program that is superior in terms of both accounting for what the old research program explained *and* discovering something new can replace a theory that has been provided some degree of initial empirical support. In general, we agree with this epistemological position. At the same time, however, we believe that any research program must meet some minimum threshold of empirical support before it can take on the status of a Lakatosian research program. And as we have pointed out, Olson's model rests on the thinnest crust of an empirical foundation. Outside of anecdotal evidence, Choi's state analysis constitutes most of that support.

The objective of our analysis of Olson's model must be understood in terms of this interpretation of the role of falsification. Our analytical and empirical exercise was *not* falsificationist in orientation in a traditional Popperian sense. If that were true, Brace and Cohen's argument that we failed to include some relevant explanatory variable would be a telling criticism of our work. In fact, from a Lakatosian perspective, even without that criticism our analysis would not merit serious attention because we failed to develop an alternative model explaining what Olson's model had accounted for and then presenting some new findings. Rather, by challenging the one major piece of empirical analysis underlying Olson's model using his own definitions of its key concepts and his own methods, we were arguing that *the construct had yet to meet the initial burden of proof required of it or any other theory.* Falsification is an appropriate analytical tool in such cases and is quite different from traditional Popperian falsification. If theories were not required to meet some initial burden of proof, we would have a proliferation of "research programs" of the flimsiest sort that would be, at least for a time, shielded from serious scrutiny. To our knowledge —and fully admitting the theoretical importance of his work—no one has yet supplied the evidence to meet this burden of proof in regard to Olson's model.

None of this, of course, should be taken to imply that it is not important to develop more complex specifications of the determinants of economic growth. While Olson himself is somewhat ambiguous about the importance of such exogenous variables, we are not. At several points, Olson (1982, 80; 1983, 34) strongly cautions against ad hoc explanations of the sort that Brace and Cohen suggest. But at other times, he remains open to specific forces operative over a more limited time span (1983, 51). As noted in our conclusion, we clearly do believe that other variables are on their face important in conditioning the range and pace of economic growth a state can experience. Our point, however, is that the burden of proof for Olson's model lies with Olson and his students. Until he or someone else develops a well-specified model—with valid and reliable measures of all of its concepts—that provides firm support for its major hypotheses, Olson's theory will remain in the limbolike category of the interesting but untested. This was our point, and we still believe it to be a valid summary of the state of research on Olson's model.

Contradictions and Trade-offs

At this point, readers might notice that there is something of a contradiction between the two arguments made by Brace and Cohen. On one hand, they suggest that we have incorrectly specified Olson's model by including an irrelevant explanatory variable, the relative power of business and labor groups. But on the other hand, they suggest that we have misspecified Olson's model by not including a potential host of exogenous variables. The two arguments do not add up to a coherent critique. This contradiction raises two issues of importance for evaluating our article.

First, given that both arguments have some initial face validity, we have to be attentive to which is the most important. Of the two, we are more concerned with the first criticism. As noted above, our specific goal in the analysis was not a complete assessment of the forces underlying economic growth but rather a more narrow consideration of the single systematic piece of evidence offered by Olson in support of his model. For that narrow purpose, only the first issue is really relevant. And we have outlined our rationale for our inclusion of the relative power terms in our models while also recognizing that measures of other dimensions of pro- and antigrowth conflict might also be tested using our decomposed specification of Olson's construct.

Second, and more importantly, we believe that the contradiction underlying Brace and Cohen's critique is ubiquitous in analyses of Olson's work. To date, Olson's model has attracted considerable empirical attention, which is not unexpected, given the theoretical importance of his analysis. But Olson's model is devilishly difficult to confront with data. A full and complete test of his model would involve pooled time series and cross-sectional analysis using more complex measures of interest group power, such as those we employed in our effort to evaluate Olson's model. Further, the test would have to be embedded in a more completed specification of the forces influencing economic growth of the type suggested by Brace and Cohen in their second criticism.

At this point, however, there is no single test of Olson's model that meets all of these requirements. Instead, we have a number of attempts to provide for a more complete specification of the determinants of economic growth, either through introduction of control variables to address specific, alternative substantive hypotheses (e.g., Brace 1988; Brace and Baumann 1988) or examination of Olson's construct in the context of a general macroeconomic model (Brierly and Feiock 1988). Some of these tests even employ pooled analysis. But they invariably use excessively simple measures of interest group power, raising the possibility that they are failing to tap the core of Olson's argument. Alternatively, there is research such as ours, which employs much better measures of these central concepts but fails to control effectively for the other forces influencing economic growth. Thus, the real dilemma, or trade-off, confronting research on Olson's model is not the one outlined by Brace and Cohen but that between specification and measurement error. To date, no one has solved both problems simultaneously. And this failure is one reason we limited the conclusions drawn from our analyses to the falsificationist function outlined earlier, rather than making broader—but ultimately insupportable—claims about the true nature of economic growth. Most of the analysts who have grappled with Olson's model have not been so circumspect.

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Notes

1. Earlier tests by Olson and Choi (Choi 1979, 1983; Olson 1982, 1983) used time as a proxy for the number and influence of interest groups. For the empirical literature on interest groups and economic growth in the states, see references in Gray and Lowery (1988).

2. For a more extensive discussion of the pooling procedure used to separate temporal from cross-sectional variation in the economic performance of states, see Brace 1988 and Brace and Baumann 1988.

3. For the specification and estimation of models involving pooled cross section and time series data, see Judge et al. 1988, Kmenta 1971, Maddala 1977, and Stimson 1985. The model we used is a covariance model and the notation is from Kmenta 1971 (p. 630). We used ordinary least squares to estimate the parameters because it generates unbiased and efficient estimates for covariance models (Kmenta 1971, 631).

4. For an attempt to develop a measure of the power of government relative to the aggregate power of all interest groups in the states, see Brace 1988.

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PUNISHING LIBERALS OR REHABILITATING LIBERALISM?

In the September 1988 issue of this Review, *Stanley C. Brubaker argued that liberals strive for neutrality concerning how people should live and that this moral ambivalence prevented them from punishing. In this Controversy, Ronald Terchek denies that liberalism is crippled by moral relativism or incapacitated for punishment. In return, Brubaker defends his firm* no *to the question, Can liberals punish?*

According to some accounts, many, if not most, of the problems that trouble modern society can be traced to the principles of liberalism. Recent criticisms have found it hollow at its core (Unger 1975), hedonistic (Strauss 1953), and bereft of virtue (MacIntyre 1981); and now Stanley C. Brubaker (1988) argues that it is unable to provide a theory of punishment. He presents us with a theory of punishment that moves beyond the liberal conception of punishment as deterrence to one that emphasizes "attitudes of resentment and indignation [that] direct anger toward the person *responsible* for the crime." This anger, we are approvingly told, allows us to "seek revenge" (p. 823). Part of the claim that liberals cannot punish is built on the case that they cannot become appropriately angry because liberalism has taken "out of political life the things men and women love most and thus would be most likely to become angry about—God, country, virtue." In their place, liberalism offers "the relatively tame concerns of individual, material well-being" (p. 825).

Linked by the author to Richard Posner and Robert Nozick, John Locke is presented as someone who wants to use state sanctions merely to protect individual private property. Brubaker goes on to claim that the efforts of Kantians and neo-Kantians (particularly Rawls) to rescue liberalism from its Lockean roots leads to an abstract account of human nature that denies people a world "where the virtuous flourish and the vicious suffer" (p. 831). The present status, or pathology, of liberalism is traced by the author to equality, which, he argues, leads to a "latent nihilism." Having lowered its horizons, liberalism is said to sponsor a "leveling" of moral standards and is unable "to defend itself against those who would violate its laws" (p. 833).

Brubaker's gloss on liberalism ignores a growing, robust literature about the moral basis of early liberalism. The research on John Locke is particularly helpful in reconstructing both his writings and intentions as well as teaching us something about how early liberals thought about both rights and a moral life. To claim that the liberalism traced to Locke has no regard for "God, country, virtue" is to miss the basic arguments of this research demonstrating the importance of moral standards in Locke's writings. Dunn (1969) finds him best understood as a Calvinist theologian; Tully (1980), as a thinker working closely with natural law; Coleman (1983), as a moral philosopher; Tarcov (1984), as an architect of a moral education; Yolton (1970), as a moral thinker who seeks to justify responsibility for action; Grant (1987), as a teacher of our natural capacities as well as our limits; Shapiro (1986), as seeking a grounded reason for moral choices. The list goes on. Whether the scholarship is friendly or not, virtually all of it moves beyond the view of Locke as an amoral figure or an apologist for materialism. The scholarship on early liberalism has not been confined to Locke: Donald Winch (1978) gives us an Adam Smith preoccupied with virtue; Bernard Semmel offers *John Stuart Mill and the Pursuit of Virtue* (1984); and Melvin Richter's (1964) earlier study provides a sensitive account of T. H. Green's view of a moral life. What is very clear from this scholarship is that early contributors to liberal theory were concerned about morality, saw liberalism as strengthening or fostering morality, and insisted that a moral life was open to everyone—but held that a moral life could not be imposed on individuals and warned that there could be no morals test in allocating rights.

If one did not take account of this recent scholarship, it would be easy to see Locke as preoccupied with private property and its protection; but even then it would be difficult to understand what he means when he tells people to live by a standard other than sensual gratifications and when he teaches people to "deny themselves" (1968, 138). For Locke, the basis for the good life is found in a rigorous moral education and the Bible. And along the way, he reminds his readers that a life that ignores patriotism mimics the life of a cow at pasture.

Brubaker's gloss on liberalism is also unable to explain Adam Smith's constant appeals for virtuous life or his claim that his view of virtue shares more with Plato, Aristotle, and Zeno than with hedonistic accounts (1976, pts. 6, 7). Nor can a view of liberalism as a justification for a self-involved, material life give us much help in understanding John Stuart Mill's account of a virtuous life and his warnings about the "money-getting and money-having" of his generation. Each of these early liberals continually warns about a preoccupation with materialism and a narrowly construed self-interest. Brubaker's idea that liberalism is built on "the relatively tame concerns of individual, material well-being" (p. 825) is simply not true. That some modern liberals may think otherwise is not sufficient warrant to claim that the core of liberalism is defective.

If Brubaker's general account of liberalism is suspect, so are his specific arguments about liberal "theory" of punishment. It is true, as Brubaker claims, that liberals primarily justify punishment as a deterrent to harmful actions that endanger rights (although some liberals offer additional reasons for taking punishment seriously). The deterrence position is based on the premise that as moral equals we carry an obligation to respect the rights of others and should expect others to respect our rights. This is no hollow or murky theory of punishment. If I violate your rights, I am liable to punishment; and for every liberal I know, this occurs within the context of settled law.

We are told by Brubaker that we should be angry when our standards of "God, country, virtue" have been violated and should call on the law to punish those who have violated these standards. Although most liberals find these are worthwhile standards, they would depart from how Brubaker links these standards to punishment. First, they recognize no single way of practicing these values. In a liberal society, conceptions of the good are expressed in diverse ways voluntarily pursued by free agents. The family is important for early liberalism because this is the milieu in which children are educated according to the moral standards of the household (see Tarcov 1984). But families differ about where the good is found (in the Old Testament, the New Testaments, the Koran, or secular humanism?) and how it is to be practiced. That liberals honor and respect the rights of others to practice the good in individual ways hardly means that liberals have no standard of the good or have a standard with no moral grounding. Nihilism is not a consequence of tolerance, nor does tolerance in any way undermine our adherence to a strong moral position. But even as liberals differ about the particulars of the good, they share an understanding about many of its properties and about the acts that deserve punishment because they morally offend. Liberals do not argue among themselves whether acts like murder or rape ought to be punishable acts, though they may differ about the extent of punishment; and J. S. Mill's observation about the status of the good and of punishment in a liberal society can be accepted by most liberals. According to Mill, "Churches and political parties may quarrel about the teaching of doctrines, but not about the punishment of crimes. There is a diversity of opinion about what is morally good, but there ought to be none about what is atrociously wicked" (1984, 108).

Liberals would also part company with Brubaker in what constitute punishable acts, particularly his call for punishment when the standards of "God, country, virtue" are breached. Liberals hold that God, not government, appropriately punishes infractions to divine law. And excepting tangible threats to national security, such as treason and spying, liberals find that county and virtue are best served when they are genuinely and voluntarily valued and practiced, not when people are coerced into prescribed forms of behavior. The quintessentially liberal way of strengthening and enhancing moral standards centers around free speech. However much liberals differ at the margin, education and dialogue have been central to correcting errors, including those of an angry state or an angry public.

Education does not always suffice to deter acts that harm others, and liberals are prepared to rely on punishment to deter crime. An emphasis on deterrence does not mean that punishment does not bear other properties, including psychological and educative ones. An emphasis on deterrence does not mean that people ought not become angry and disgusted by criminal acts, but liberals would deny that personal grief or outrage should drive the law. Due process, fixed punishments, and equality before the law are critical to their argument.

Smith, for one, believes our rage at heinous crimes reflects what is natural in humanity, the moral sentiments. According to Smith, some offenses naturally excite resentment and make us desire that the guilty party be punished. "Resentment," he tells us, "seems to have been given us by nature for defence, and for defence only. It is the safeguard of justice and the security of innocence"; and he goes on, like other liberals, to argue that "the mere want" of virtue in others may disappoint us but does us no "mischief from which we can have occasion to defend ourselves" (1976, 176). Smith also recognizes that grief-stricken individuals often lack the even-handedness that he wants embedded in the law. The issue is not that resentment is unnatural or unwarranted in many cases; the issue for someone like Smith is that legal punishment is not primarily cathartic and that legal and procedural safeguards must guide the administration of justice.

For a liberal like J. S. Mill, the severity of punishment not only carries a deterrence effect but also *expresses the moral priorities* of a society. In rejecting the claim that capital punishment for murder shows no regard for human life, he argues, "We show, on the contrary, most emphatically our regard for it, by the adoption of a rule that he who violates that right in another forfeits it for himself" (1868, cols. 1052–53). Mill's respect for liberty hardly leads him to level moral standards or embrace nihilism.

On another occasion, Mill found that proposals to strengthen laws regarding wife and child abuse were too weak and therefore defective. Physical abuse of wives and children outrages Mill, and he wants not only to deter this practice with a strong punishment but also to *make a moral statement* about using brute strength over helpless victims. To this end, he finds that corporal punishment "is the moral medicine needed for the domineering arrogance of brute power" (1984, 105).

Now, many liberals (as well as others) would deny Mill's arguments about capital punishment for murderers or corporal punishment for wife and child abusers. The point is that there is no liberal canon or "theory" of punishment. What unites them is their concern about the importance of rights.

Many prominent liberals also find that punishment can carry *educative* consequences. Part of Mill's justification of corporal punishment for physical abuse rests on such grounds. He argues that "the beneficial fruits of such a law are not to be measured by the crime and suffering which it would directly prevent," although he thinks its deterrent effect would be considerable. Its "more extensive beneficence" comes in the moral lessons it teaches, particularly to the young. "What chance," Mill asks, "is there for education, if the schools teach one lesson, and the laws another contradictory to it?" (1984, 106). He goes on to argue, "It is of little avail to talk of inculcating justice, or kindness, or self-control, [when] the judicial and police courts teach by actions, so much more efficacious than words, that the most atrocious excesses of ungovernable violence are, in the eyes of the authorities, something quite venal" (1984, 107).

Liberals take law and punishment seriously. They know, as Mill did, that punishment can have deterrent and educative effects. But no liberal would accept Brubaker's argument that the coercive power of the state can be justified because of its cathartic effects in venting rage and anger.

His contention that the diverse understandings of the good in liberalism result in a debilitating relativism and ultimate nihilism ignores the moral concerns of early liberalism. Scholarship has sufficiently advanced to demonstrate that liberals, along with the other members of society, have a conception of the good. Liberals expect people to come to know the good in diverse ways and deny that deviation from a proscribed orthodoxy about "God, country, virtue" can serve as a basis of punishment, particularly in a society that claims to be free.

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In "Can Liberals Punish?" I distinguish punishment from related phenomena (such as taxes, reparations, and penalties) and identify it essentially with retribution, that is, giving criminals what they deserve. Though liberals can deter, restrain, and require restitution, they cannot punish because they cannot give people what they deserve. They cannot give people what they deserve because they have excluded from politics, as much as possible, the principal basis of desert—virtue, the human good, human excellence. Terchek charges me with ignoring recent scholarship on the moral basis of early liberalism and with creating a suspect liberal "theory" of punishment when in fact "there is no liberal canon or 'theory' of punishment."

I wish to respond to these charges; but to avoid confusion on what is really at stake in this debate, it is necessary at the outset to correct Terchek's misleading characterizations. First, according to him I claim that punishment is "justified because of its cathartic effects in venting rage and anger," that "personal grief or outrage should drive the law," that "anger . . . allows us to 'seek revenge.' " My argument is quite the contrary: in it anger does not justify, anger *is justified.* I point out that anger unmeasured by justice "can be spurred to fanaticism," that anger is "inappropriate" where there is excuse or justification for the harm that one did to another, that "the criminal sanction gives proper shape or form to anger," measuring it by judgments of desert—that is, that "the deed deserves the hard treatment, shame, and reprobation in proportion to the gravity of the crime. This stern measured judgment makes punishment what it is" (pp. 821, 823, 824).

Some authors, most notably Emile Durkheim, do come close to justifying punishment because of the salutary effects of the expression of anger: "[Punishment's] real function is to maintain inviolate the cohesion of society by sustaining the common consciousness in all its vigor" (1984, 63). But that is not my argument. That anger expressed in punishment may serve the function identified by Durkheim, that the satisfaction of measured anger is both healthy and proper, that by reflecting on this anger we may learn something about the justification for punishment—with all of this I would agree. But as a justification, anger in my argument plays a derivative, not a primary role.

Second, according to Terchek I call upon the government to enforce divine law. ("Liberals would also part company with Brubaker in what constitute punishable acts, particularly his call for punishment when the standards of 'God, country, virtue' are breached.') He attributes to me the proposition that "deviation from a prescribed [religious] orthodoxy" warrants the criminal sanction; but since I do not address the general question of what acts should constitute punishable offenses, on what grounds he makes this claim is far from clear, especially since he cites nothing to support it. Terchek perhaps draws from the point I do make—that punishment itself, in the retributive sense, transcends commonly invoked liberal limits on the use of criminal sanctions—to infer that if those limits are crossed in the act of punishment, they may be crossed in the definition of criminal acts. That inference is not unreasonable; but implying—as I do—that limits may be redrawn is far from concluding—as I do not—that limits should be abandoned in the name of some religious orthodoxy.

Third, one might conclude from Terchek's critique that my focus was on Locke and British liberalism. In fact, in dealing with actual attempts by liberals to account for punishment, this liberalism fills one column, versus ten dealing with Kant and the neo-Kantians. I give the Lockean tradition less attention because, though it is more resistant to nihilism, it is, unfortunately, less influential in contemporary academe. It is also more forthright about criminal sanctions. As Locke maintains, "Reparation and restraint" (the latter term obviously includes deterrence and education) are the only justifications (1952, 6–7, 8). As I pointed out in n.1 of my original article, I could not deal with all varieties of liberalism and so explicitly excluded those that attempt to build mildly robust theories of the human good within liberalism (William Galston and Rogers Smith among contemporaries and John Stuart Mill among the earlier liberals).

Fourth, the obsession with the phrase "God, country, virtue," is Terchek's, not mine. I used it once, parenthetically, to indicate what liberalism, in contrast to preenlightenment theories, chose not to place at the center of political concern. The phrase seems to have proved overly provocative for Terchek, who appears both compelled to defend liberalism before it and outraged that anything aligned to such concerns should be implicated in applying the criminal sanction. This compulsion and outrage, in fact, seem to underlie his two primary charges.

His first charge, that I ignored recent scholarship on Locke (and other early liberals), is true. I ignored it because none of it refutes my thesis that Locke and his intellectual progeny justify the criminal sanction exclusively for the purposes of reparation and restraint. Indeed, it is hard to see how even the most subtle of arguments could detract from Locke's plain words to this effect. Regarding Terchek's broader point that Locke was more concerned with morality and less concerned with property than I indicate, I would point out that while the words *morals, morality,* and *ethics* do not appear in the *Second Treatise,* the statement, "Government has no other end but the preservation of Property" (1952, 53–54) does.

To be sure, Locke's understanding of property is generous, and certainly one can speak of a "moral life" necessary to secure property and other natural rights or even of a strictly circumscribed definition of the "good life," understood as comfortable self-preservation and the esteem of others. But anything higher would hardly be compatible with his comments on the limits of human knowledge: "The Mind has a different relish, as well as the Palate; and you will as fruitlessly endeavor to delight all Men with Riches or Glory . . . as you would satisfy all Men's Hunger with Cheese or Lobsters. . . . Hence it was, I think, that the Philosophers of old did in vain enquire, whether Summum Bonum consisted in Riches or bodily Delights, or Virtue, or Contemplation. And they might have reasonably disputed, whether the best Relish were to be found in Apples, Plums, or Nuts; and have divided themselves into Sects upon it" (Locke 1979, 269). No doubt Locke is, as Terchek suggests, "concerned about morality." My question is whether this particular understanding of morality can authorize retribution. Locke says it cannot.

Locke, at least, is consistent. If human reason is incapable of apprehending goods beyond comfortable self-preservation and the esteem of others, rationality must consist in acting according to rather narrowly conceived cost-benefit assessments. Deterrence is rational because it is forward-looking: it increases the costs of future undesirable actions. Retribution is not rational, for it looks backward to what was done and why it was done; it terminates in an expression of disapproval and reprobation and a marking of the criminal's standing in relation to human excellence.

But Terchek might wish to press the point further. If liberalism does enable a decent, moral life of sorts and can authorize deterrence, he might argue, why even consider retribution, that is what was done and what does the criminal deserve? Why not limit ourselves to what people believe will happen to them if they commit a crime? The problem is that by this logic sanctions disproportionate to the offense or the exemplary suffering of innocents portrayed as guilty would make good deterrents. Locke's own meditations reveal a further cynicism. In *The Reasonableness of Christianity* he famously criticized the ancient philosophers for merely showing the "beauty of virtue," while "leaving her unendowed," that is, without showing a more definite payoff for being virtuous. The "reasonableness" of Christianity, for Locke, consists largely in its endowing virtue with the promise of an afterlife of rewards and punishments. With Christ's promise of afterlife, "interest is come about to her [virtue]; and virtue now is visibly the most enriching purchase, and by much the best bargain" (1958, 70). But Locke understands God's rationality as he does man's, that is, in terms of forward-looking cost-benefit analysis. "Rationally," as Locke was led to acknowledge in his private journal, God need do nothing (or could do anything) with man in the afterlife (King 1830, 229–30); it is only necessary for people to *believe* they will get what they deserve.

Though space permits only an allusion to the problem here, one must further wonder how stable the moral life in Locke's thought (supposedly extolled in the recent literature cited by Terchek) can be without the moral foundation necessary for retribution. While Locke does write, especially in *Some Thoughts Concerning Education,* of the virtues needed for the citizens who would inhabit the state portrayed in the *Second Treatise,* it emerges that these virtues originate and terminate in men and women's proud desire to show that they are free (see, e.g., 1968, 173–74). Yet freedom is expressed not as any longing for completion in the beauty of virtue or the fullness of wisdom, but as an aspect of power, that is, as a means, not an end (1979, 233–87). And while Locke's skillful channeling of this proud desire for freedom promises decency, power without purpose can hardly terminate, in the long run, elsewhere than in a will to power.

Without commenting in detail on Terchek's contention that recent scholarship supports his view that the good life for Locke consists in living according to the Bible, I would invite him to consider recent works by Pangle (1988), Kraynak (1980), and Zuckert (1986), all showing how Locke uses the trappings of biblical and traditional morality to clothe his revolutionary teachings in legitimacy and at the same time loosen people's attachment to these ancient moral cynosures. That Locke succeeded in this endeavor exceptionally well is indicated by the fact that in defending liberalism against my "leveling" charge, Terchek is forced to repair to its early advocates.

Terchek's second charge is more confusing. He maintains that I misportray the liberal account of punishment, for "there is no liberal canon or 'theory' of punishment. What unites [liberals] is their concern about the importance of rights." But this concern for rights is exactly what I took as central in developing the Lockean, neo-Lockean, Kantian, and neo-Kantian accounts of punishment. Retribution either transgresses, or cannot be authorized by these rights. Much of the tone in this section derives from Terchek's outrage at my supposed eagerness to punish deviators from religious orthodoxy. Once that is scraped away, we are left with statements from Adam Smith and John Stuart Mill designed to show that liberals really can punish and do so properly. The statement from Smith can be understood to refute my argument only if one misunderstands me to say that anger is a sufficient justification for punishment. The statement from Mill is more interesting, and Terchek describes it as both *educative* and *expressive.* If it is the former, it does not refute my argument, for education (despite Terchek's attempts to distinguish them), is just an aspect of deterrence. If it is "expressive," at least in the retributive sense that I use the term in my essay (p. 825), the statement is at odds with Mill's "one very simple principle" of liberty, by which the power of the state must not be exercised except for the prevention of "harm to others" (Mill 1959, 13). Also, as I point out above, to the extent that Mill does imply a hierarchical understanding of the human good, he falls outside the purview of my analysis.

As should have been evident, especially from the introductory and concluding comments in my essay, I write as an admirer, friend, and beneficiary of liberal democracies, who thinks that they remain—so far at least—both deeper and healthier than can be accounted for in liberal theory. I welcome Terchek's attempt to rehabilitate liberalism. But we must realize that the strength and worth of liberalism came from outside it, from the moral capital of ancient virtue. What remains of this we should reinvest to perpetuate the many blessings of liberty.

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DEMOCRATIC THEORY AND THE PUBLIC INTEREST: CONDORCET AND ROUSSEAU REVISITED

Bernard Grofman and Scott Feld argued in the June 1988 issue of this Review *that Jean-Jacques Rousseau's contributions to democratic political theory could be illuminated by invoking the theorizing of one of his eighteenth-century contemporaries, the Marquis de Condorcet, about individual and collective preferences or judgments. Grofman and Feld's claims about collective consciousness and the efficacy of the public interest provoke debate. One focus of discourse lies in the application of Condorcet's jury theorem to Rousseau's theory of the general will. In this controversy David M. Estlund and Jeremy Waldron in turn raise a variety of issues of theory and interpretation; Grofman and Feld then extend their argument, and propose clarifications.*

While few things are more controversial than the nature of democracy, there is nonetheless something of a received view, namely, that a proper democratic vote expresses the voter's preferences about the social choices in question and that these are to be combined in an outcome that pleases as many individuals as possible within the bounds of fairness. The consensus around this model of democratic voting is an odd thing, stemming as the model does from Arrow's well-known proof that no way of combining such votes can simultaneously satisfy several apparently reasonable criteria, a theorem rooted in the work of the Marquis de Condorcet, who first demonstrated the possibility of cyclical majorities. Condorcet, however, also produced an equally stunning but less well-known result, namely, that majority rule can make a group more likely to give correct answers than the average member or even than the most competent member.

These are landmark results, each pertinent to a different model of democracy. But Condorcet's famous result about a *preference conception* of democracy, the paradox of cyclical majorities, is negative; it is a problem for that model, which becomes clearest in Arrow's (1963) masterful elaboration. Condorcet's other landmark result, the jury theorem, is a positive result for the other conception of democracy, what we might call the *epistemic conception* (following Cohen 1986; Coleman and Ferejohn 1986). It is surprising that the epistemic conception of democracy has not been pursued and developed in light of this promise, while the preference conception has come to dominate democratic theory despite (or, perversely, because of?) its long-known problems.

The imbalance is beginning to be redressed in two ways. In the first place, researchers have recently rediscovered and vastly extended Condorcet's jury theorem results. In the second place, there is a growing appreciation of the contributions to an epistemic conception of democracy that were made by Condorcet's great contemporary, Rousseau. These two lines of work have been recently brought together in a fascinating Condorcetian reading of Rousseau by Grofman and Feld, "Rousseau's General Will: A Condorcetian Perspective" (1988). I wish to raise some questions about their interpretation of Rousseau and then consider whether the epistemic model of democracy can easily avoid the Arrowian difficulties faced by preference models.

Rousseau and Condorcet

Grofman and Feld's central interpretive point seems indisputable. Rousseau conceived voters as giving their opinion on an independent matter of fact—the content of the general will—and held that the answer receiving a majority of votes under certain circumstances was guaranteed to be correct. This feature of Rousseau cries out for a Condorcetian interpretation. However, the issue arises whether more of Rousseau's views can also be covered by the Condorcetian umbrella.[1]

For example, Grofman and Feld interpret Rousseau's worries about factionalization as owing to the Condorcetian consideration of reducing the effective number of voters. If individuals vote in blocks, the effective number of voters is reduced from the number of individuals to the number of blocks, and this reduces the competence of the group, other things being equal. In particular, the reduction of group competence depends on the assumption that the competences of block voters will not be greater than their competences if they had voted independently. For example, if a faction chooses to follow an opinion leader rather than vote independently, the assumption is that the competence of the opinion leader is either always or on average no higher than the average competence of the other members of the group if they were independent. Condorcet's theorem shows that group competence increases or decreases with the number of independent voters if average competence is the same. If the opinion leaders of all factions are, in effect, made the only voters, then if the average competence of the voters in the general procedure is the same as it would be with all independent voters, group competence is reduced.

But if, instead, the "party line" within groups is determined by majority rule within the faction, the Condorcet jury theorem can perhaps be brought to bear to show that group competence can exceed the average individual competence. For example, the opinion leader may be chosen by majority rule, in which case the opinion leader may, on average, have a higher-than-average competence for Condorcetian reasons. Indeed, the faction need not even choose to defer to one of its members; and under certain conditions and in relatively small groups, the faction's competence can even exceed that of the most competent member (Grofman 1978). Would this situation still reduce group competence?

On the contrary, it might seem that group competence must simply be *increased.* After all, the competence of every individual becomes just that of the group itself; and this is higher than average or even higher than *any* individual's competence. So many or all individuals increase their competence in such cases, and no one's competence is decreased. Apparently, then, the average competence within each faction—and so in the group as a whole—is increased. Since the number of voters is unchanged, the Condorcet theorem should say that group competence is higher than it would have been without these democratic factions.

However, this line of reasoning neglects the fact that block voting, where there is correlation among individual votes beyond what would normally be expected simply from similar independent competences, effectively reduces the number of voters (Owen 1986). The problem is in determining whether this competence-reducing factor is more powerful than the competence-increasing factor of the higher competence of such democratic factions. Which group is more competent: one with a larger effective number of voters of a certain competence or one with a smaller effective number of voters of higher competence? Obviously, it depends on the differences in numbers and the differences in competences, but this question has not, to my knowledge, been thoroughly studied.[2] Until there is further study of democratic factions, it cannot be assumed that factionalization, as such, reduces group competence. All that can be said is that nondemocratic factions tend to reduce group competence.

Now this would be enough for Grofman and Feld's interpretive purposes if the factions Rousseau is concerned with were internally nondemocratic. Are they? Rousseau says, "When intrigues and partial associations come into being at the expense of the large association, the will of each of these associations becomes general in relation to its members and particular with in relation to the state" (*Social Contract* 2.3). If the factions have their own general wills, as he says, then his view must be, as it is with a whole community, that the general will is best discovered by majority rule. This might seem to favor the mini-democracy view of factions over the Grofman-Feld interpretation of deference to opinion leaders.

However, the minidemocracy version presented above depended on everyone's addressing him- or herself to the issue of the common interest of the whole society. Only then can the faction's deliberation raise the faction's competence *on the common interest of the whole society* above the average member's. Rousseau's view of factions is clearly different. The question factionalized voters address is, What is in *our* [the faction's] common interest? And when a party line is drawn and toed in the general voting procedure, the same interpretation of factionalized votes is appropriate: "X is in the common interest of my faction." This counts against interpreting Rousseau in the minidemocracy way discussed above, but it also counts against Grofman and Feld's interpretation. The problem is not—at least not *solely*—that the number of voters is effectively reduced. It is at least as important that factionalized voters are not addressing the proper issue. This is clear in the words "general in relation to its members, *and particular in relation to the state.*" Elsewhere Rousseau discusses the advanced stages of factionalization and decay: "Everyone, guided by secret motives, no more express their opinions as citizens than if the state had never existed; and iniquitous decrees having as their sole purpose the private interest are falsely passed under the name of laws" (*Social Contract* 4.1). And in the extreme case of deference to another, selling one's vote for money, "The error he commits is that of changing the thrust of the question and answering a different question from the one he was asked. Thus, instead of saying through his vote *it is advantageous to the state, he says it is advantageous to this man or that party that this or that view should pass*" (4.1). The dichotomous choice between states of affairs *S* and *T* is not sufficient to determine what judgment is expressed by a vote. *S* is not a judgment "*S* is in the common interest" is a judgment. If a jury model, that is, a Condorcet model, is appropriate, the dichotomous choice voters face is between judgments. Rousseau claims that the judgment choice that factionalized voters address is different from that faced by others. Where most are addressing "*S* is in the common interest of the whole society" versus "*S* is not in the common interest of the whole society," members of faction *F* are addressing "*S* is in the common interest of *F*" versus "*S* is not in the common interest of *F*." Clearly, where individuals are not addressing the same issue, Condorcetian considerations do not apply, and there is no pooling of wisdom. The problem is not mainly the reduced number of effective voters; it is the failure of the voters to address the same issue. At least this is the most defensible difficulty with factionalization until the effects of democratic factions are better understood.[3]

Still, Rousseau does mention that under factionalization "there are no longer as many voters as there are men" (*Social Contract* 2.3). Its relevance, however, may be that since there is a pervasive tendency to err in the direction of particularity—to bias one's vote in the direction of one's private interest—these small errors can be overwhelmed by large numbers of others with countervailing biases.[4] Factionalization, especially at an advanced stage with only a few factions, precludes this corrective influence.[5]

Public Deliberation

Grofman and Feld's claim that the effective reduction of the number of voters reduces group competence ought to be limited to (internally) nondemocratic factions, as I have argued. This adjustment calls for a further adjustment in their interpretation of Rousseau. Grofman and Feld seem to misinterpret the following remark of Rousseau's: "If, when a sufficiently informed populace deliberates, the citizens were to have no communication among themselves, the general will would always result" (*Social Contract* 2.3). Rousseau is held by Grofman and Feld to see "the 'deliberative process' as one taking place within individuals rather than in terms of a process of group debate" (1988, 569). And they say, "Each voter is polled about his independently reached choice, without any group deliberation" (p. 570). Their text is not entirely determinate on this point, but there is some reason to think Grofman and Feld may have taken Rousseau to be claiming that voters *ought not* to have any communication among themselves regarding the issues to be voted on. This would seem to concede too much to the totalitarian, anti-civil liberties, interpretations of Rousseau.[6] *Too much* because it is not demanded by the text. Rousseau says only that *if* there were no communication (and other conditions are met), the procedure would be infallible. This does not imply that noncommunication is a necessary condition for discovery of the general will. In fact, it does not even imply that it is necessary for *infallibility* of the discovery process, though Rousseau may well believe this. We saw above that there is no Condorcetian basis for the claim that factionalization is detrimental but only for the claim that a certain kind of deference is. Similarly here, deference is the culprit, not communication. The notion of deference is hard to make precise; but where there is no communication, there certainly can be no deference nor any of its ill effects. However, if deference can be avoided, communication would seem to have advantages from a Condorcetian perspective. Increasing the information of a nondeferential voter would tend to increase the voter's competence, and this can only increase the chances of the group's ascertaining the general will. A Condorcetian reading would be embarrassed if Rousseau had criticized public deliberation about voting. Fortunately for Grofman and Feld, these comments about communication are not such a criticism. As mentioned, it is not entirely clear how Grofman and Feld read Rousseau on this point. In fairness, they do once gloss him in an unobjectionable way: "Thus, each voter is seen as seeking to reach an *individual and independent* judgment about alternatives" (p. 569, emphasis added). Admittedly, a Rousseauian theory must place great weight on individual independence; but if it is to remain Rousseauian, independence must be distinguished from asociality.[7]

Rousseau and Arrow's Conditions

Grofman and Feld point out that the Condorcet-Rousseau interpretation of votes as judgments rather than expressions of preference is an important alternative to the dominance of preference interpretations of voting at least since Arrow (1963). One might legitimately wonder, then, whether Arrow's proof about the impossibility of an acceptable aggregation procedure can be avoided on this alternative view. Grofman and Feld believe that the common view that Arrow's work casts doubt on claims involving such concepts as the common good or the general will "is simply wrong. Our position, like Rousseau's, is that the general will may exist but that the outcome of any voting process is but an imperfect reflection of it" (1988, 574, n. 17).

Their view seems to be that Arrow's problem affects only voting procedures; so that while attempts to constitute or discover a common good, general will, and so on through voting procedures are rendered problematic, the *notions* of common good, general will, and so on are not. However, the application of Arrow's work is not limited to voting procedures. There is no *function* from individual rankings to an aggregate ranking that meets Arrow's conditions. Therefore, doubt *is* cast on those notions insofar as they are taken to be *functions* of individual rankings.

Rousseauian theory in particular does not take the general will to be a function of actual individual *votes*, but it may yet—and I believe does—take the general will to be a function of individual *rankings*, namely, interest rankings. It is not necessary to take up the issue of Rousseau interpretation here. The point can be made by merely raising this possibility. Suppose the content of the general will is the common interest (on socially significant issues). Surely the common interest must be some function of individual interests, probably representing what is in the interest of *every citizen.*[8] The general point is that if it is any function of individual rankings, Arrow's arguments apply. More particularly, if the function in question is the common interest function, Arrow's conditions are likely to be violated in the fact that no matter how "in *S*'s interest" is interpreted, some sets of individual interest rankings will not exhibit any common interest, in violation of the Arrowian condition of unrestricted domain. This, then, is an application of Arrow's theorem not to a voting procedure but to an account of the relation between the common good and individual goods. The general will, the common good, and so on are not insulated from Arrow's problem unless they are not defined as functions of individual rankings; but Grofman and Feld have not made any such case.

It would be hasty to disabuse political thought of these notions on that basis alone, however, since there is room for debate about whether Arrow's conditions must be accepted in this context. For example, suppose certain profiles of individual rankings were inconsistent with their all being members of a genuine political community. Then it would be nonsense to say that the difficulty of aggregating profiles of noncommunity rankings shows that there is no such thing as the good of a community. This is just an example of how the application of Arrow to the common good itself rather than to voting procedures might open new challenges to the conditions he imposes. In this hypothetical case, the challenge would be to the condition of unrestricted domain, which requires that a function return an outcome for absolutely any set of individual rankings. Why suppose that the notion of the common good depends on the possibility of a procedure that could aggregate *any* profile, even a noncommunity profile of individual rankings? Of course, this is not a substantial challenge unless some account of the difference between community and noncommunity profiles has been provided. It is meant here only as an example.

While my remarks have been largely critical, I have only offered them because of the promise of Grofman and Feld's approach to democratic theory. The work of these two authors and others along Condorcetian lines is potentially momentous for democratic theory; and the Condorcetian reading of Rousseau is, I believe, accurate in important respects. The recent elaborations of Condorcet's work on the competence of groups is a significant advance toward Rousseau's goal of governing communities according to the truth despite the absence of any philosopher-king to reveal it.

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Grofman and Feld (1988) have outlined an exciting and illuminating interpretation of Rousseau's theory of the general will. They argue that many of the things Rousseau says in *Social Contract* about how the general will can emerge from popular voting may be understood along the lines of Condorcet's jury theorem. That theorem holds that if voters are addressing a common question with two answers, one correct and one incorrect (e.g., whether someone is guilty or innocent or which of two alternatives better promotes the general good) and if the average probability of each voter choosing the correct answer is greater than .5, the probability that the answer chosen by a majority of them will be the correct one increases to certainty as the size of the group increases (Condorcet 1976, 33–70).

For brevity of discussion, I define *competence* as follows: the competence of a person or group with regard to some pair of answers to a question (one correct, one not) is just the probability that given a choice, the person or the group will choose the correct alternative. So the theorem simply asserts a mathematical relation between member competence and group competence: if the average competence of members of a group is greater than .5, the competence of the group deciding by a simple majority (majority competence) approaches 1 as the number of members increases.

I want to point out one or two difficulties in the use of Condorcet's theorem as an interpretation of Rousseau's theory of the general will, and indicate how that interpretation might be supplemented to provide a more realistic and attractive image of Rousseau's politics along Condorcetian lines.

Though I shall be pointing out some features of Rousseau's theory that are not especially congenial to this approach, I should emphasize that this should not be taken as a criticism of Grofman and Feld's suggestion. *Social Contract* is a complex and paradoxical work, and there is probably no chance of fitting everything Rousseau says into the framework of a single, coherent theory. We can only examine whether the features of Rousseau's thought that are independently attractive and theoretically important are best understood along the lines that Grofman and Feld suggest. In other words, the question to ask is whether the Condorcetian suggestion helps us to *interpret* Rousseau's theory of politics by making it the best it can be (see Dworkin 1986, 52–53).

The first thing to note about the Condorcet theorem is the importance of the assumption that average voter competence is greater than .5. The theorem does *not* make group competence an increasing function of average member competence for *any* value of the latter. The same reasoning that yields Condorcetian optimism about the general will also yields the conclusion that if average voter competence dips below .5, majority competence tends towards zero as group size increases. In other words, the theorem is one that faces two ways: it provides reason for optimism about voting if individual voters are on average more likely to be correct than not; but it provides reason for pessimism if on average they are more likely to choose incorrectly.

It may be thought odd that a voter's competence could be less than .5. After all, even a voter who chooses at random should choose correctly half the time. Unfortunately, as Condorcet himself pointed out, randomness is not the worst we can expect: "In effect, when the probability of the truth of a voter's opinion falls below 1/2, there must be a reason why he decides less well than one would at random. The reason can only be found in the prejudices to which this voter is subject" (1976, 62). It is a well-known theme in the critique of democratic politics that correct decisions are often those that seem counterintuitive or run contrary to the preconceptions with which the common man approaches ordinary decisions (e.g., Plato, *Republic* 487b–502c). So it is quite possible for unenlightened people, who (naturally enough) employ their preconceptions and intuitions in political decision making, to do worse than randomizers.

Now although Grofman and Feld mention Rousseau's belief that popular voting on matter of the general good will occasionally be mistaken (*Social Contract* 2.3), they offer Condorcet's theorem as an interpretation of what they take to be his confidence that in *most* cases the vote of a popular assembly will coincide with the general will (1958, 569). But since, as I have pointed out, the Condorcet theorem faces both ways, it cannot by itself provide an interpretation of this confidence unless there is reason to attribute to Rousseau an independently grounded belief that average voter confidence in a citizen assembly will be higher than .5.

As a matter of fact, it seems that Rousseau did not hold such a belief. Certainly, he was pessimistic about the chances of citizens' being able competently to address questions of the general good in large and populous societies (*Social Contract* 2.9). There are good reasons for this pessimism.

First, the more populous a country, the more complex the issues that arise and the less clear and evident the solutions. The point made earlier about preconceptions obviously applies more acutely the more complex the social problems that have to be faced are.

Secondly, Rousseau suggests that an increase in size may make it difficult for voters to identify with the general good in such a way as to prevent their decision making from being distorted by bias and self-interest: "Each individual, having no taste for any other plan of government than that which suits his particular interest, finds it difficult to realize the advantages he might hope to draw from the continual privations good laws impose" (*Social Contract* 2.7).

It is no use, by the way, responding to this that the Condorcet theorem already presupposes that citizens are voting for the general good as they see it and not on the basis of self-interest. The disjunction between the general good and the interest of individuals is too easily exaggerated in Rousseau exegesis. The general will really is supposed to will a good that is *common to all*, a good that serves the individual interest of *everyone*. That is the basis of Rousseau's contractarianism (*Social Contract* 1.7, 2.4). Moreover, distortion caused by uninformed self-interest is, on the Rousseauian view, one of the prime causes of voter incompetence. A competent decision maker is guided by the aspects of personal interest that correspond to the interests of everyone else in society; an incompetent one is guided by aspects of personal interest that are socially dissonant. If increasing population size makes it more difficult for voters, with the best will in the world, to discern which of their interests are common to their fellow citizens and which are not, it is all the more likely that they will come up with incorrect answers when they (think they) are addressing themselves to questions of the general good.

The first two points suggest that voter competence might be inversely correlated with the size of the population whose interests are to be taken into account in answering questions about the general good. A third reason looks more closely at the composition of the decision-making body itself. For a country of a given population, it is likely that the larger the popular assembly, the lower the average level of education and enlightenment among its participants. This is not an a priori truth; it rests, however, on the politically realistic assumption that those who have the vote in a restricted franchise are likely to be more educated than those admitted to it when the franchise is extended (Mill 1975). The point is one that Rousseau hinted at in his remarks about 'the common herd' (*Social Contract* 2.7). Intriguingly, it is also a point that Condorcet emphasized in his presentation of the theorem:

> This conclusion leads first of all to a rather important observation. A very numerous assembly cannot be composed of very enlightened men. It is even probable that those comprising this assembly will on many matters combine great ignorance with many prejudices. Thus there will be a great number of questions on which the probability of the truth of each voter will be below 1/2. It follows that the more numerous the assembly, the more it will be exposed to the risk of making false decisions. (1976, 49)

This, of course, is most interesting. The Condorcet theorem states that where average voter competence exceeds .5, the more numerous the assembly, the greater the probability that a majority decision will be right. But the present observation tells us that the more numerous the assembly, the lower the competence of voters on average is likely to be. Although there is no reason to think that mathematically, the two factors exactly cancel one another out, still, it is worth noting that the very thing that enhances majority competence, given a fair level of average competence, tends also and independently to reduce average competence.

There are good reasons, then, for being pessimistic rather than optimistic about the application of the Condorcet theorem to a political assembly in a society of any size. And, in fact, Rousseau did not exhibit the wholehearted optimism about majoritarian outcomes that Grofman and Feld impute to him. By definition, the general will is always correct; but even when they turn their attention purely to that end, the people, on Rousseau's account, are unlikely to get it right:

> The people, being subject to the laws, ought to be their author: the conditions of the society ought to be regulated by those who come together to form it. But how are they to regulate them? Is it to be by common agreement, by a sudden inspiration? . . . How can a blind multitude, which often does not know what it wills, because it rarely knows what is good for it, carry out for itself so great and difficult an enterprise as a system of legislation? *Of itself the people wills always the good, but of itself it by no means always sees it.* The general will is always upright, but the judgment which guides it is not always enlightened. (*Social Contract* 2.6, emphasis added)

The sentence emphasized makes it clear, I think, that the source of error is *not* that voters are asking themselves the wrong question (and therefore coming up with, say, the will of all rather than the general will), they are asking themselves the right question (What is for the general good?). But Rousseau simply has no confidence that real-world voters addressing this question will come up with the right answers.

How might this difficulty be dealt with? Does this Rousseauian pessimism about voter competence not undermine the theory of the general will precisely on Condorcetian grounds? To mitigate the pessimism we have to consider whether there is any way voter competence might be enhanced in a Rousseauian polity. Four possible strategies spring to mind.

The first is to limit the range of questions that are put to a popular vote: the larger the population, the more limited the range of questions. This was Condorcet's solution:

> Now since these prejudices and this ignorance can exist in relation to very important matters, it is clear that it can be dangerous to give a democratic constitution to an unenlightened people. A pure democracy, indeed, would only be appropriate to a people much more enlightened, much freer from prejudices than any of those known to history. For every other nation, this form of assemblies becomes harmful, unless the assemblies are limited in the exercise of their power to decisions directly relating to the maintenance of security, liberty, and property: matters upon which a direct personal interest can adequately enlighten all minds. (1976, 49)

As we noted earlier, the competence of a voter on an issue of the general good is likely to be greater in cases where there is a direct and evident connection between the general good and personal self-interest.

Something rather along these lines is involved in Rousseau's insistence that it is the task of the government (i.e., the magistry, the administration), not the sovereign (i.e., the people), to make decisions about particular persons and events (*Social Contract* 3.1). The people as a whole are to deal only with matters that are universal in character and that therefore touch them all because in the state's dealings with any particular person or event, the competence of everyone but the party immediately affected is likely to be limited (2.6).

The second strategy (also proposed by Condorcet) is less likely to be congenial to Rousseau. It is that the political participation of the common people should be limited to the choice of representatives:

> But it can be observed that in the majority of matters submitted to the decision of an assembly, the same voters whose opinions have such a small probability of being true can be enlightened enough—certainly not to pronounce with some probability of truth as to which man among a great number has the most merit—but to choose, as the most enlightened, one of those whose opinions will have a large enough probability of being true. Thus a numerous assembly who are not very enlightened could be usefully employed only to choose the members of a less numerous assembly to whom the decision on other matters would then be entrusted. (1976, 61)

Of course, even this will work only if the masses can be relied on to choose representatives more enlightened than themselves and not to be induced to vote for those who share the prejudices that constitute their own incompetence.

In any case, Rousseau would have no truck with such a strategy, given his well-known opinion that representation is the end of freedom and that 'every law the people has not ratified in person is null and void' (*Social Contract* 3.15).

This makes it surprising that the third strategy (which Rousseau *does* propose) seems to involve an even more substantial derogation from participatory politics than the choice of representatives would involve. The solution he favors is the introduction into politics of "a superior intelligence . . . beholding all the passions of men without experiencing any of them," a "sublime reason" with recourse to "an authority capable of constraining without violence and persuading without convincing"—a lawgiver on the model of Moses or Solon or Lycurgus to set the agenda for legislation and organize the people into a state (*Social Contract* 2.7). This is not the place to comment on Rousseau's interpolation of such a deus ex machina save to say that it seems to move his theorizing into a mythic dimension where it is no longer clear whether he is genuinely attempting to address the issue of citizen competence as a problem in real world politics.

The fourth strategy is one that Rousseau toys with. It is very prominent elsewhere in the tradition of participatory democracy, but its relation to the Condorcet Theorem is not very well understood and is misstated, in my opinion, by Grofman and Feld. This is the role that may be played by debate and discussion among the citizens in enhancing the competence of those who participate.

The Condorcet Theorem requires that the votes cast by participating citizens be *independent* of one another. (If they were not, one would, roughly speaking, have to multiply voters' competences to calculate group competence, diminishing the probability of a correct majority decision for any average competence less than 1.) Grofman and Feld interpret this independence condition as follows: "A group of size N chooses between any two alternatives by means of a majority vote in which each voter is polled about his or her independently reached choice, without any group deliberation" (1988, 570). They maintain that this corresponds to Rousseau's view of "the deliberative process as one taking place within individuals rather than in terms of a process of group debate" (p. 569).

In fact, Rousseau is ambivalent on this. The passage they cite is translated as follows: "If, when the people, being furnished with adequate information, held its deliberations, the citizens had no communication one with another, the grand total of the small differences would always give the general will, and the decision would always be good" (*Social Contract* 2.3). But *communication* here may not mean what we understand by participatory dialogue and debate. In the immediately following sentence, deliberation without communication is contrasted not with discussion as such but with intrigues and the formation of factions. In other words, *communication* in this passage may mean something akin to what *conversation* means in phrases like *criminal conversation*—a suspect and unhealthy connection of selfish interests rather than high-minded political dialogue. It is true that later in the work Rousseau suggests that "long debates, discussions, and tumult proclaim the ascendancy of particular interests and the decline of the State" (4.2). But this can be read in context as a pathology of dialectic intractability rather than as a pessimism about discussion as such. Elsewhere he talks happily enough of "public deliberation" (1.7) and "common deliberations" (2.4); and it is hard to see why an actual *assembly* of the people in one place (as opposed to a poll that might be conducted door-to-door) would be thought necessary if discussion were dangerous or unimportant (3.12–15).

Moreover, if the Condorcetian approach precludes discussion, it is entirely at odds with the spirit of the participatory tradition in which Rousseau's political theory is usually located. Since Aristotle's dictum, "Nature . . . does nothing without some purpose; and for the purpose of making man a political animal she has endowed him alone among the animals with the power of reasoned speech" (*Politics* 1253), the participatory tradition has placed great emphasis on dialogue among citizens both as an intrinsic good and as a way of improving the likelihood that they will decide wisely on the issues they are addressing. In fact Aristotle provides an early version of the Condorcet theorem in what he calls 'the argument of the collective intelligence of the masses, so long as they do not fall below a certain standard. Each individually will be a worse judge than the experts, but when all work together, they are better, or at any rate, no worse' (1282a).

How does discussion among citizens enhance voter competence? More importantly, can it do so in a way that does not undermine the independence condition of Condorcet's theorem?

There are various ways in which the first question might be answered. One answer rests on an optimism about the impact of those who are enlightened. Even if the average competence of the members of a group is below .5, it is likely that there will be some members whose competence is much higher. If so, they may be able to persuade those of lesser competence to abandon their prejudices and show them why (for example) an apparently counterintuitive proposal is not as inimical to the general good as it might seem. Their arguments may be accorded some respect by the less-competent, in part for the reasons Condorcet identified in the second strategy (representation): people can often recognize others more competent to solve a problem than they are. Condorcet thought this an argument for isolating the more competent in a special assembly. But exactly similar reasoning yields the possibility of their being able to improve the average competence of those who would be at least sufficiently enlightened to choose them as representatives.

That apart, there is also the possibility that if one source of average incompetence is unfamiliarity with the range and complexity of a problem, bringing citizens together in an assembly where they can be exposed in discussion to a range of perspectives other than their own may make it more likely that reason rather than prejudice will prevail as they address the problems of a large society (Mill 1975, 197–88).

Finally, there is simply the optimistic assumption that prejudice fares less well than reason in open debate. If people are at all open to argument, we would expect discussion to diminish the probability that voters would subsequently do less well in choosing between alternatives than they would if they chose randomly. (But admittedly, this *is* optimistic; we are equally familiar with the way prejudices can be confirmed and compounded in discussion).

If there is a chance that discussion among the citizenry enhances average competence, does that nevertheless undermine the Condorcet result by compromising its condition of voter independence? Unlike Grofman and Feld, I think the answer is *no*.

We must remember that dependence and independence are, in this context, mathematical ideas; and not everything we call interaction or lack of interaction in politics can be taken as an interpretation of them. The sort of interaction between voters that would compromise independence would be interaction in which voter *X* decided in favor of a given option just because voter *Y* did. Grofman and Feld rightly recognize that the formation of factions and retinues—a process Rousseau deplored (*Social Contract* 2.3)—would have this effect (1988, 571). But *X*'s being persuaded by *Y* in argument or holding itself open to such persuasion does not in itself involve *X*'s deciding to vote one way rather than another *because of* the way *Y* is voting.

Grofman and Feld suggest that independence is vitiated by *any* positive correlation among individual choices beyond the correlation to be expected from similarities in competence alone (1988, 571). That is ambiguous. It is too strong if it requires the individual competences to be independent of one another (nothing in Condorcet's reasoning presupposes *that*). But if it categorizes the impact of one person's arguments on another as a (developed) similarity in competence, it does not preclude the possibility of mutual enhancement of competence through discussion.

It does not matter, for Condorcet's argument, whether or not individual *competences* are independent of one another. His theorem makes group competence a function of average individual competence irrespective of how the latter is generated. There is an obvious sense in which the competences of individual voters cannot possibly be independent of one another if they are members of the same society. They read the same newspapers, argue in the streets and coffee shops, and so on. What matters, for the purposes of independence, is what happens when the competence is exercised. In order to apply Condorcet's theorem to predict majority competence, we are going to have to decide the point at which the average competence will be calculated. For best results, it is obvious enough that it should be calculated at the moment just before the vote is taken. Now it is almost trivially true that if we allow discussion to take place between the time average competence is calculated and the time the vote is taken, our application of the theorem may go awry. But that has nothing whatever to do with independence. It is simply that average competence may change in the meantime, and we will have to do our calculations of majority competence over again using new values for the variables. Independence is a condition on the way competence is expressed in voting, not a condition on the way competence is generated. If average voter competence is calculated *after* a discussion in the assembly has taken place, one cannot object to a Condorcetian prediction *based on that calculation* on the grounds that the competence of some voters was affected by that of others.

I conclude, then, that Grofman and Feld have exaggerated the atomism of voter deliberation required for a Condorcetian interpretation of Rousseau's political theory. By misinterpreting the independence condition, they make Rousseauian politics, on this interpretation, look much less attractive as a conception of collective choice than it has to. The background difficulty remains, of course: even though competence-enhancing discussion is not ruled out, there is no guarantee that it will carry average competence to a level where majority voting produces positive results. Still, I think it a happy coincidence that restoring the interactive dimension of participation offers the best hope that the Rousseauian theory of politics may get the benefit rather than the detriment of Condorcet's two-faced theorem.

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In the light of the issues raised by Estlund and Waldron, we are pleased to have the opportunity to clarify and expand upon our discussion (Grofman and Feld 1988) of what has been called the "epistemic" concept of democracy (Cohen 1986; Coleman and Ferejohn 1986) and of the links between the views of Rousseau and those of Condorcet. We share with Estlund and Waldron the view that the proper questions to address are not esoteric points about textual exegesis but rather fundamental questions central to political theory, such as, What is the public interest and how can we design institutions that will serve it?

We, Estlund, and Waldron are in agreement on many other points. In particular, we agree that—in Estlund's phrasing—"Rousseau conceived voters as giving their opinion on an independent matter of fact, the content of the general will, and held that the answer receiving a majority of votes under certain circumstances was guaranteed to be correct"; and these authors agree with us that the Condorcet jury theorem can be used to provide insight into how and when majority decision making can be used to ascertain the general will. We are also in agreement that members of the group majority can go wrong in two ways: by addressing the wrong question (i.e., concerning themselves with their own particularized interest rather than the general interest) or by lacking sufficient competence to discern the general will despite the best intentions.

Estlund emphasizes the difficulties posed for a Condorcetian view of Rousseau's general will posed by the first type of error; Waldron emphasizes the need to clarify the circumstances under which errors of the second type can be avoided. Both Estlund and Waldron provide a number of salient quotes from the writings of Condorcet and Rousseau that help to clarify the relationship between the ideas of these two figures of the enlightenment. Both call attention to areas where the discussion in our earlier essay needs to be more clearly stated and elaborated. We see this response less as a rebuttal than as an expansion of points that were either omitted or left obscure in our previous article.

The Estlund essay makes three important claims: The first is that an epistemic concept of democracy (i.e., one that is judgment-based rather than preference-based) does not avoid the problem of potential inconsistencies in group decision making posed by Arrow's famous impossibility theorem. The second is that the existence of cohesive subgroups (factions) actually might, under some circumstances, improve the reliability of group decision making rather than making it worse. The third is that group deliberation might make group decision making more accurate than what would be expected from the purely "statistical" effect of the Condorcet jury theorem, which is a variant of what in probability theory is known as "the law of large numbers."

Waldron, like Estlund, raises the issue of the effect of group deliberation on group judgmental accuracy, but he also raises an important fourth question not considered in our earlier essay: Can we specify the conditions under which (mean) group competence can be expected to be above one-half? We find Waldron's observations about this question to be both useful and provocative. Moreover, he closely ties his discussion of this question to points raised by Rousseau and Condorcet themselves.

The General Will Revisited

The concept of the public interest has come under critical challenge from economists in the past several decades. Buchanan and Tullock (1962) make the claim for a methodological individualism within whose framework the idea of a public interest distinct from some aggregation of individual preferences can only be viewed as nonsensical. For Buchanan, the only legitimacy that can be attached to social institutions arises from their being instantiated as part of a package (constitution) that has been given unanimous consent. After the work of Paul Samuelson (1947) economists (with the possible exception of those in the cost-benefit tradition) have eschewed interpersonal comparisons of utility and opted for an ordinalist foundation to social welfare theory. Advocates of the criterion of (Pareto) efficiency often reject any attempt to choose among points on the Pareto frontier in terms of other normative standards.

However, it is Arrow's impossibility theorem that is widely seen as the finishing blow to the claim that the public interest can be meaningfully defined. Roughly speaking, Arrow (1963) demonstrates that no rule for the resolution of conflicting individual preferences among a set of three or more alternatives that is based on ordinal preference rankings and is responsive to changes in individual preferences in a sensible (monotonic) fashion can guarantee that group preferences among the alternatives will be transitively ordered unless we impose constraints on what preference orderings voters are allowed to have. (For a clear and relatively nontechnical introduction to Arrow's theorem, see Riker 1982; see also Plott 1976.)

Estlund asserts, "Surely the common interest must be some function of individual interests, probably representing what is in the interest of *every citizen.* The general point is that if it is any function of individual rankings, Arrow's arguments apply." In note 8 Estlund calls attention to a relevant quote from *Social Contract* (2.1), a quote we also cited: "Were there no point of agreement among all these [private] interests, no society could exist." Similarly, Waldron asserts, "The disjunction between the general good and the interest of individuals is too easily exaggerated in Rousseauian exegesis. The general will really is supposed to will a good that is *common to all,* a good that serves the individual interest of *everyone.*"

This line of argument, which treats the public interest as the interest that is in common among individuals, is very similar to the Runciman and Sen (1965) approach, which looks at the public interest as the cooperative solution to an *n*-person prisoner's dilemma game. While we regard this as a plausible interpretation of the general will, we do not regard it as the whole story (as we noted in passing in our previous essay) for three main reasons.

First, the public interest may be other than the commonality of interests among the particular set of individuals who presently comprise a society. We take seriously Burke's remark about society's being a contract "between those who are living, those who are dead, and those who are to be born."

Second, and even more importantly, we believe that it is possible for individuals to address the question, What is the common good? not by thinking about consequences for themselves but rather by focusing on the abstract properties of systems of rules (or equivalently, systems of institutions). This, for example, we take to be the heart of the Rawlsian enterprise.[9] In this context we would, however, emphasize that the search for a general will presupposes a minimal commonality of basic values as well as what we might normally think of as common interests. Without such commonality the quest for a common Rawlsian "reflective equilibrium" is doomed to be futile. Moreover, Barry (1964, 14) reminds us that in Rousseau's view there were "a number of requirements that had to be met before it (the general will) applied at all: political virtue (reinforced by a civil religion), smallness of state, and rough economic equality among the citizens."[10]

Third, and relatedly, we agree with the point made by Rousseau (and reiterated by Waldron) that the general will may only be defined for certain (very limited) types of choices, namely, those dealing "with matters that are universal in character" (*Social Contract* 2.6). Barry (1964, 14) correctly points out that in Rousseau's view, "if only some are affected by a matter the General Will cannot operate." As Waldron reminds us, Rousseau insisted that "it is the task of government (i.e., the magistracy, the administration), not the sovereign (i.e., the people), to make decisions about particular persons and events" (*Social Contract* 3.1).

Of course, in practice, it may be very difficult to distinguish between rule creation and rule application, since few rules (laws) are apt to cover all eventualities. And as was evident in the last revision of the income tax code, rules that seem general may in fact be tailor-made to apply only to one individual (or corporation) or a handful of individuals (or corporations). Nonetheless, it seems sensible to us to emphasize the connection between the public interest, constitutional design, and the rule of law (see Redford 1958) but to reject the claim that the public interest can be defined in all choice situations.

As yet, however, we have not addressed the heart of Estlund's claim that appeal to a public interest, however defined, cannot really get us out of the problems for social choice posed by Arrow's impossibility theorem. To do so we must explicate the link between our notion of the public interest and transitive collective choice.

For situations in which we can posit that individuals are by and large seeking to implement some common notion of the public good (or some other widely held value) in deciding among alternatives, we specify properties of a matrix of margins that are likely to arise even if individuals differ in their competence at correctly perceiving which of any two alternatives is higher on the criterion variable. For reasons of space we only sketch the nature of the argument. When these matrix conditions are satisfied, choice will be transitive.

A matrix is said to satisfy *Borda margins* if there is a way of ordering alternatives such that in every row, above the main diagonal, margins increase (or at least do not decrease) as we move to the right and such that in every column, above the main diagonal, margins increase (or at least do not decrease) as we move from the entry in the bottom row upward. We expect the Borda margins condition to be (approximately) satisfied in situations involving judgments because the greater the difference in "value" between two alternatives, the greater, on average, the plurality of voters who will prefer one to the other.

When a matrix satisfies the Borda margins condition, group majority choice will be transitive, and alternatives will be ordered in accordance with their Borda scores. Moreover, in a group whose preferences satisfy the Borda margins condition, as long as the group's mean competence is above .5, the Borda winner can be taken to be an indicator of the general will for that group (or to represent the probable highest value on whatever other criterion variable the group's members can be taken as trying to implement [see Young 1986, 1988]). When the Borda margins condition is satisfied we avoid the intransitivities that are the heart of the Arrowian impossibility result.[11]

In work now in progress we are looking at data on voter preference orderings in single transferable vote elections in labor unions and other organizations to determine whether votes satisfy either the Borda margins condition or the condition of ideological margins.[12] It is our belief that in many voting situations, it is reasonable to treat voters as if they were, on balance, trying, albeit imperfectly, to find the choice that is "best" with respect to some shared set of values. This can be true even if some voters completely lack this motivation and no voters have only this motivation. Our ongoing work on Borda margins is intended to provide both a theoretical and an empirical meaning to the idea of "judgmental" decision making even in contexts where relatively few voters can be viewed as exclusively oriented toward a search for the common interest. This work parallels our earlier efforts to define a notion of collective ideological consistency that is distinct from simply counting how many individuals see the world primarily in ideological terms (Feld and Grofman 1986a; 1986b; 1988).[13]

Factions within Groups and the Search for the General Will

Estlund suggests the possibility that especially when each subgroup's vote is decided by a majority vote within the subgroup, it may be preferable for a group to allow subgroups to influence the votes of their members. "After all," says Estlund, "the competence of every individual becomes just that of the [sub] group itself; and this is higher than average, or [perhaps] even higher than *any* individual's competence." Estlund is raising an important point: under what circumstances will some other rule than a simple majority decision maximize the group's judgmental competence? This is a complicated topic about which we have written extensively (Feld and Grofman 1984; Grofman 1975, 1978, 1979; Grofman and Feld 1984, 1986; Grofman, Feld, and Owen 1982; Grofman, Owen, and Feld 1983; and Shapley and Grofman 1984; see also Nitzan and Paroush 1982, 1985; Owen 1986; Urken and Traflett 1984; Young 1986, 1988). Here we will summarize the practical import of some little-known technical results and work out a nine-voter example in detail.

The key result is one due independently to Shapley and Grofman (1984) and Nitzan and Paroush (1982, 1985) but mathematically identical to a well-known result in the electrical engineering literature. Let the ith voter have competence p_i, then (among the very large class of voting rules that can be represented as weighted voting games) the decision rule that optimizes group judgmental competence when voters are choosing independently is one that assigns weights to each voter proportional to the log-odds of that voter's competence (i.e., if p is the probability of a correct judgment, the log-odds ratio is log $[p/1 - p]$). (This can result in negative weights for those with competence less than .5, but we shall act as if everyone has at least a chance probability of being correct.)

We consider the effects of factions, first in terms of groups whose members have identical voter competence, then in terms of groups whose members vary in competence. We focus on one particular type of factional decision making within subgroups, an internal majority rule process within subgroups, that is, situations where subgroups use majority rule for decisions within the subgroup and then subgroup members vote as a bloc.

Consider situations with all voters having identical competence, p, with $p > .5$. If there are nine voters, divided into three subgroups, Estlund would suggest that in this situation, because the three-member subgroups have increased their competence, this gain in competence may be enough to compensate for the reduction of the "effective" number of decision makers from nine to three. By using the Shapley-Nitzan-Grofman-Paroush log-odds theorem, we can see that Estlund's proposition is necessarily false. For groups whose members have identical competencies, if $p > .5$, simple majority rule (i.e., equal weighting) is optimal. However, the reduction in overall competence that comes from using what Owen (1986) refers to as "indirect" majority rule (i.e., majority rule both within and between subgroups) will, in this example, be relatively small—a maximum difference of slightly over two percentage points.[14]

Other forms of subgroup decision making fare even worse than the division of groups into three-member subgroups considered above. In particular, if there is a (democratic) majority faction, the judgmental competence of the group becomes identical to that of the majority faction. Especially for small groups, cutting the effective size of the group nearly in two can have substantial consequences. For example, for $p = .6$, in a nine-member group the difference in competence between simple majority rule and rule by a (democratic) majority faction is .051 (.7334 − .6826), while for $p = .8$, the gap is .04 (.9814 − .9420).

However, the worst effect of factionalism comes when there are cabals within cabals, pyramiding to majority control. In this worst-case scenario, a group of two can control a subgroup of three that in turn controls a subgroup of size five that can control a group of size nine, and so on. Here the majority competence of the group is reduced to the average competence of its members, no matter how large the group.

For the case where all voters have identical p values, our discussion emphasizes the competence-reducing possibilities of factionalization. In addition, we strongly agree with the point made by Estlund (citing Rousseau) that, to the extent that there are subgroups, the focus of decision making may shift from the general will to the will of parts of society. Even if these parts are large—indeed even if one of them is a majority faction—subgroup interests and those of the larger society need not coincide (as Madison pointed out in *Federalist* 10). We also share Waldron's view that the worst evil of subgroup decision making is that it is likely to shift the focus of concern from the general will to the will of all.[15]

As noted above, when a group's members have identical values of p, if p is greater than one-half, simple majority rule will always be preferred to any form of subgroup voting. The situation is more complex when the members of the group differ in their competencies. The Shapley-Nitzan-Grofman-Paroush log-odds result tells us that simple majority rule is generally not the optimal rule for groups whose members vary in their competence. Nonetheless, the optimal group decision rule is likely to be only a slight improvement on simple majority except for small groups in which there is a considerable variance in the competence of the group's members.

One such example is a nine-member group, three of whose members have a competence of .9 and six of whom have a competence of .6. In this example, simple majority rule has a competence of .91—considerably lower than the .97 competence of the group under the optimal rule that assigns a weight of 2.20 (ln[.9/.1]) to the members with .9 competence and a weight of .41 (ln[.6/.1]) to the members with .6 competence. Only a unanimous vote of the six members with .6 competence should be allowed to overturn a 2–1 vote of the members with .9 competence.

Of course, if the most competent member of the group is much more competent than the average member, deference is often sensible. For example in a nine-member group with eight members of competence .6 and one of competence .9, the eight should defer to the one unless *at least six* of the eight feel otherwise.

In general, a multistage process will lead to better choices only under the special condition when it leads to (effectively) greater weights for more competent individuals. In the next-to-last example above, if the three most competent individuals formed a majority coalition with two of the others, the overall competence of the group would be .94, better than simple majority. However, most factional arrangements would only hurt the group competence. For example, if five of the less-competent members form the majority coalition, the group would only have a competence of .68.

Group Deliberation

When group members differ in their competencies, simple deference to the most competent member or members of a group may not be appropriate (as we have seen), in that a weighting of votes that gives some (reduced but nonzero) weight to the less-competent group members may actually yield a higher overall competence for the group. These examples bring us naturally to the question of the effects of group deliberation on group judgmental accuracy.[16]

Deliberation can have a number of effects. It provides information about who holds what preferences and diffuses information about why people hold the preferences that they do. In the process of discussion, motivations toward either a public-regarding or a private-regarding ethos can be enhanced. From discussion we may learn that others are (or are not) looking primarily to their own self-interest, which may motivate us to do likewise. Learning which preferences given individuals hold, we may use this information as a cue to what is truly in our own (or the common) interest (see Grofman and Norrander n.d.).

Group deliberation raises a number of questions that take us into empirical issues beyond what we can deal with here. To what extent are individual perceptions of their own competence accurate? To what extent can individuals identify those who are more knowledgeable than themselves? Are there particular types of issues in which judgment-improving forms of deferences are more likely? On decisions where individuals see themselves as not competent to judge, how likely are they simply to abstain? Is there an optimum size for deliberating groups?[17] and perhaps most importantly, To what extent can the sharing of information through the process of group discussion led to improvements in individual accuracy[18] and are there ways to structure the deliberative process (e.g., via a "Delphi"-type interaction like that investigated by Dalkey and his colleagues at RAND, [Dalkey 1969a, 1969b] or related procedures like the nominal interactive method [Gustafson, Lebeq, and Walster 1973]) that can significantly improve the accuracy of group decision making? The one thing that we can be sure of is that there is a broad social consensus that deliberation is desirable in the democratic process, although we must also recognize that sometimes group deliberation is only a charade (as it often is when legislators pontificate before empty chambers solely for the record or witnesses testify at hearings before legislators who have already made up their minds).[19]

Mean Group Competence

Waldron correctly points out that the more-heads-are-better-than-one expectation that can be drawn from the Condorcet jury theorem is tied to the assumption that (mean) group competence is above .5. Competence values above .5 are important more generally. Implementing the Shapley-Nitzan-Grofman-Paroush optimal weights requires us to be able to specify the competencies of the group's members with the expectation of higher weights for the more competent members of the group. Even if exact competencies are unknown, as long as negative weights are prohibited, we would optimally be assigning positive weights only to those believed to have a competence value above .5. What reasons do we have to believe that groups will have mean competence above .5 or, indeed, any members with competence significantly above .5?

Waldron offers a number of different plausible solutions to this problem. For example, he suggests (following Condorcet) that we might limit the operations of sovereign democratic decision making to areas where individuals may be expected to be both motivated and competent to discern the public interest or (following Rousseau) that we might limit the sovereign to matters that are universal in scope where particularized interests will have least play (as we have previously noted). We find these suggestions quite plausible.

Also, Waldron believes that deliberation may improve individual judgmental accuracies to the point that assuming a mean competence of .5 may not be unreasonable. While we, too, find this line of reasoning quite plausible, we have to express our fears that there is only limited empirical evidence that deliberation improves outcomes beyond the purely statistical effect produced by the law of large numbers when initial mean competence in the group exceeds one-half. As noted above, deliberation can have a variety of effects, some positive and some negative.[20]

Another option considered by Waldron to improve judgmental accuracy (also suggested by Condorcet) is to use the voting process not to make choices among alternatives but to select a subgroup of individuals more competent than the voters themselves. Certainly, it seems reasonable to believe that in many circumstances voters can identify others more competent than themselves to judge certain issues.[21]

A fourth line of attack is to emphasize the need to train citizens to make informed judgments on public issues and to be loyal to certain shared values and identity. As noted in our previous essay, Rousseau's view that voting could lead to the general will was dependent on (1) the voters being informed and (2) voters subordinating their self-interests to a search for what is in the common interest. Although no one would wish to rely on civic virtue alone, few (even among economists) would wish to live in a society whose members had been raised with no moral beliefs other than the belief that no moral (or patriotic) claim should be permitted to outweigh self-interest narrowly defined.

Both Rousseau and Condorcet, in their writings, expressed great concern for this moral dimension of citizenship. Rousseau discusses this topic in *Emile* as well as in his essay *The Government of Poland;* Condorcet does so in a number of essays, some of which have not been yet translated into English. Shklar 1969 is a useful reference for a discussion of Rousseau's views on this point, while Kintzler 1984 is devoted entirely to explicating Condorcet's views about public education and citizenship. Both Rousseau and Condorcet (especially the former) can be thought of as advocates for what has been called a "civil religion."

Justifying Representative Democracy

While *representative* democracy can be justified in terms of the practical impossibility of a pure democratic form or by the sorts of fears about mob rule that motivated many of the founding fathers and led them to devise a government whose powers were divided, limited, and balanced, our approach leads us to emphasize the benefits of bringing the specialized skills of knowledgeable individuals to bear on society's problems. In Condorcetian terms—in some situations the most useful voting that can be done by a large assembly is to select a smaller and more competent subgroup to decide matters. Even if we conceive of the public interest as some aggregation of the interests of society's parts, it may still be the case that judgment is needed to ascertain those subgroup interests as well.

As Estlund correctly points out, the "received view" of democracy is "that a proper democratic vote expresses the voter's preferences about the social choices in question, and that these are to be combined in an outcome that pleases as many people as possible within the bounds of fairness." It is this preference-based notion of social choice that is reflected in Arrow's theorem and the vast literature that has sprung up from it, all of which can trace its lineage back to Condorcet's 1785 *Essay* and the paradox of cyclical majorities there first identified. We have argued that a judgment-based notion of choice is an appropriate complement to the preference-based perspective.

We certainly do not wish to make the absurd claim that the interests of individuals in a society never conflict or that all, or even most of, politics is about the search for the public interest. Rather, much of the normal process of government deals with the reconciliation of conflicting preferences (see Mansbridge 1980, 1981). However, we do wish to rebut the claim that politics is only about the search for power or advantage. A vision of politics (and politicians) that is blind to the shared values in a society can never hope to explain the course of history.[22]

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Notes

1. Grofman and Feld broach the subject of possible historical influences between Condorcet and Rousseau. They point out that Condorcet's *Essay on the Application of Mathematics to the Theory of Decision-making* makes no mention of Rousseau and suggest that Rousseau might have had access to Condorcet's ideas by way of their mutual friend D'Alembert. But there is evidence Rousseau influenced Condorcet rather than the other way around. The doctrine of the general will, including the central respects in which it is susceptible of Condorcetian analysis, was present in Rousseau's 1755 article in Diderot's *Encyclopedia* on the subject of moral and political economy (known now as the *Third Discourse* or *Discourse on Political Economy).* The relevant doctrines are scattered throughout that article, but much can be found in paragraph 23. It implies that the general will can be

known in the right conditions by assembling the people. Even though *Political Economy* contains no explicit discussion of voting (and so none of majority rule), we may presume that that would be the point of such public assemblies and their "deliberations." The same paragraph includes the doctrine that the decision of the assembly can fail to be the general will; and elsewhere in *Political Economy* (esp. par. 16) the reason is said to be that citizens often substitute private or partial interests for the common good in their deliberations. This suggests that the correct answer is objective in the way a Condorcetian analysis would require. The relevant Condorcetian elements are, then, already present in this early article.

Rousseau's *Social Contract* appeared in 1762. Rousseau could not have known of Condorcet via D'Alembert until those two became associated in 1765, three years after the appearance of *Social Contract* and ten years after *Political Economy* (Baker 1976, xxix).

Condorcet, on the other hand, definitely knew of Rousseau's work by 1785 at the latest. In that year (or before) he published some anonymous notes and an introduction to a speech he had given in 1781 accepting induction to the French Academy. In those notes he mentions Rousseau with approval, though he cites no specific doctrines (see his note E in Condorcet 1976, 20). Seventeen eighty-five is also the year of the *Essay*, 30 years after Rousseau's *Political Economy*, 23 years after *Social Contract*, 7 years after Rousseau's death, and 4 years after the appearance of the *Confessions*, whose posthumous appearance inspired a popular cult in France (see Miller). An active intellectual like Condorcet was most likely aware of the *Social Contract* well *before* the 1785 notes, but by themselves the notes establish the possibility of Rousseau's influence on the *Essay*.

Since Rousseau could not have known of Condorcet through D'Alembert before the *Social Contract*, since Condorcet almost certainly knew of the *Political Economy* and the *Social Contract* long before the *Essay* and absolutely certainly knew of his work in the year of the *Essay*, and since there is an extraordinary affinity between the *Social Contract* and the *Essay* (demonstrated by Grofman and Feld), it seems highly likely that Rousseau was an important influence on the topic and argument of the *Essay*.

2. See, however, Grofman 1975 for some discussion of related issues.

3. In Estlund n.d. I argue that interpretations of votes as expressions of individual preferences fail to account for the requirement of the idea of democratic voting that all address some single issue. So the desirability of addressing the same issue stems not just from Condorcetian considerations but also from constraints on what should *count* as democratic voting.

4. This may provide an alternative to Grofman and Feld's reading of the vexing "pluses and minuses cancel each other out" passage of Rousseau's (1988, 572).

5. Grofman and Feld are aware of this point about factions' addressing the wrong question (1988, 573, n. 5, 12), but they underemphasize it. They interpret Rousseau's worries about factionalization wholly in Condorcetian terms when the failure of factionalized voters to address the common good is at least as good an explanation.

6. Grofman and Feld rightly criticize certain traditional interpretations of Rousseau which hold that he subordinates the importance of individuals to the greater social whole. There is an explicit text that supports Grofman and Feld's position in this important long-standing controversy:

> Is the welfare of a citizen any less the common cause than the welfare of the entire state? If someone were to tell us that it is good that one person should perish for all, I would admire this saying when it comes from the lips of a worthy and virtuous patriot who dedicates himself willingly and out of duty to die for the welfare of his country. But if this means that the government is permitted to sacrifice an innocent person for the welfare of the multitude, I hold this maxim to be one of the most despicable that tyranny has ever invented, the most false that one might propose, the most dangerous one might accept, and the most directly opposed to the fundamental laws of society. (*Political Economy* 31)

7. Beyond the fact that the passage does not require that reading, there is some positive evidence that Rousseau *advocated* prevote discussion in 1764 in "Letters from the Mountain." (I am grateful to Joshua Cohen for suggesting that support could be found there.) For example,

> In a republican state where French is spoken it is necessary to make a separate language for government. For example, *to deliberate, to opine, to vote,* are three very different things that the French do not sufficiently distinguish. *To deliberate* is to weigh the pros and cons. *To opine* is to state one's view [dire son avis] and give reasons for it. *To vote* is one's suffrage, when nothing remains but to count the voices. One begins by putting the matter under deliberation. The first time around, one opines; finally, one votes. (Rousseau 1964, p. 833, n.; translation mind, with help from Meg Denton)

It is unclear whether *deliberer* means collective or individual weighing of pros and cons. Like the English *deliberation*, it can mean either. Notice that Rousseau does not say that one begins by deliberating, which would be the closest parallel to *one opines* and *one votes*. Instead, he says "one begins by putting the matter under deliberation," suggesting that the deliberation is not individual but collective. In any case, he explicitly says that one states and motivates one's view before voting, enough to constitute pre-vote communication. Still, it is a central challenge for a Rousseauian philosophy of democracy to account for public discussion and majority rule in a way consistent with individuals' thinking for themselves, one of Rousseau's central and persistent concerns.

8. See *Social Contract* 2.1: "Were there no point of agreement among all these [private] interests, no society could exist. For it is utterly on the basis of this common interest that society ought to be governed."

9. There is, however, a question of whether Rousseau would permit consideration of the public interest to take place behind a Rawlsian veil of ignorance. According to Barry (1964, 13) Rousseau insists that the policy's effect on the voter "must actually be in prospect."

10. However, we, like Barry, believe that a meaningful concept of public interest can be defined even in the context of the large, modern nation-state, not just for a city-state such as Rousseau's Geneva.

11. It is well known that Arrow (following Black 1958) proved a possibility theorem for single-peaked preferences. Sen (1966) specified a set of restrictions on individual preference orderings necessary and sufficient to guarantee transitivity of social choice. Gaertner and Heinecke (1978) and Feld and Grofman (1986a, 1986b) have, in effect, restated those conditions in terms of what the latter refer to as "net preferences." In the Gaertner and Heinecke and Feld and Grofman approach, it is not necessary that any type of preference ordering be excluded. What is necessary is that preferences over any set of three alternatives cancel each other out so that the only preferences orderings that remain (the net preferences) give rise to a transitive ordering.

Feld and Grofman (1986a, 1986b, 1988) show that voter preferences over a set of four possible presidential candidates in 1980 satisfied the net preference condition and gave rise to orderings that were single-peaked with respect to an underlying left-right continuum: Kennedy-Carter-Ford-Reagan. A sufficient condition for what we call "net single-peakedness" is the ideological margins condition. It requires that the matrix showing the margin by which alternative i would defeat alternative j in a paired contest between only those two alternatives (negative if j loses to i) has the properties that there is a way of ordering alternatives such that in every row, above the main diagonal, margins increase (or at least do not decrease) as we move to the right *and* such that in every column, above the main diagonal, margins increase (or at least do not decrease) as we move from the entry in the top row downward. This line of research on ideological margins parallels, for preferences as opposed to judgment, our work on Borda margins.

12. See n. 11.

13. Also see n. 11.

14. The only circumstance under which the outcome of majority voting among the three groups of three and simple majority among the nine voters can differ is when fewer than five voters shape the outcome of indirect majority rule. But this can occur only when the votes within the three subgroups are 2-1, 2-1 and 0-3. The probability of this split occurring if indirect majority rule reaches the wrong answer is $27p^4 (1 - p)^5$; the probability of this split occurring if indirect majority rule reaches the correct answer is $27 (1 - p)^4 p^5$. Thus, after some arithmetic, we see that the advantage of simple majority rule over indirect majority rule (three groups of three), for the nine-voter case with identical p values is simply $27p^4 (1 - p)^4 (2p - 1)$. For p greater than one-half this expression will be positive, but it will be relatively small (equalling zero, of course, if p equals one-half or if p equals one). For $p = .6$ the value of this expression is .017; for $p = .7$, it equals .022; for $p = .75$, it equals .016; while for $p = .9$, it equals .002.

This example may provide some insight into Supreme Court decision making when the Court is seeking to "interpret" statutory language or the language of the U.S. Constitution itself, or if we look at cases where the Supreme Court has attempted to enunciate a notion of the public interest (see, e.g., *Federal Power Commission* v. *Hope Natural Gas* 1944 and discussion in Redford 1958).

15. Effectively this is the main point made by Ted Lowi (1964) in his attack on "interest group liberalism."

16. The view that Rousseau was antideliberative, which Estlund wrongly attributes to us, does seem to be held by Walzer (1983). The "intent of Rousseau's argument" that the citizens would always reach a good decision if, "being furnished with adequate information, . . . [they] had no communication with one another," Walzer states, is to "eliminate meetings altogether and ban the clubs and parties that politicians organize to make their persuasiveness effective. . . . Then, each individual would think 'only his own thoughts.' There would be no room for persuasion or organization, no premium on speechmaking and committee skills, instead of an aristocracy of orators, genuine democracy of citizens would take shape" (p. 306). But Walzer then goes to reject this view as impractical, noting that information adequate for judgment cannot be provided except by allowing communication. "Even if we don't talk with one another, someone must talk to all of us, not only supplying facts and figures, but also defending positions" (p. 306).

17. The claim is frequently made that above or below a certain group size, group processes tend to break down. This is a claim made by Madison in *Federalist* 55 (see also *Federalist* 58).

> Sixty or seventy men may be more properly trusted with a given degree of power than six or seven. But it does not follow that six or seven would be proportionately a better depository. And if we carry on the supposition to six or seven thousand, the whole reasoning ought to be reversed. The truth is, that in all cases a certain number

> at least seems to be necessary to secure the benefits of free consultation and discussion, and to guard against too easy a combination for improper purposes; as, on the other hand, the number ought at most to be kept within certain limits in order to avoid the confusion and intemperance of a multitude.

A related argument can be drawn from the logic of the free-rider problem (Olson 1965). If shirking falls off more than linearly with group size, the gain in competence with group size identified in the Condorcet jury theorem may be outweighed by other social-psychological effects (cf. Grofman 1974).

18. One relevant model is that for tasks where different actors possess different pieces of knowledge or different types of skills. In such circumstances, deliberation and coordination is needed. Without communication, groups will fail. Metaphorically, in this model, we may think of the public interest as a kind of jigsaw puzzle. We do need to clarify one point, the meaning of the term *independent choices.* As we use that term, it has a technical meaning, namely, that the probability that voter i makes a correct choice will not be affected by whether or not voter j has made (or will make) a correct choice. It does not mean what it might mean in ordinary usage, to wit, that members of the group lack a shared background of knowledge or values. Nor does independence, per se, imply the absence of communication. Our discussion of voter independence was not as clear as it might have been. We do not actually hold some of the views Waldron takes us to task for.

19. There is a literature in social psychology that touches on these questions. A useful review is found in Hastie 1986. Bordley (1986) and Dalkey (1986) review modeling issues related to these questions, as does Grofman (1980; see esp. the references to the work of James Davis and associates on models of conformity and persuasion in jury decision making). The general problem of optimizing judgmental accuracy of groups in situations where voter choices are nonindependent is beyond our scope here. For some efforts along that line see Owen 1986, Shapley and Grofman 1984, and Grofman 1980.

20. For the special case where the formal voting rule is supramajoritarian, Grofman (1979) has shown that group deliberation can reduce group judgmental accuracy if it takes the group away from de facto majority rule. See also n. 11.

21. We do not dispute Waldron's claim that Rousseau was hostile to indirect democracy. There are important parallels between the views of Rousseau and those of Condorcet, but certainly their views of democracy are not identical.

22. If there are widely endorsed norms of citizenship, we do not need to rely solely on internal moral imperatives to motivate their implementation. Social processes can provide private incentives to reinforce norms (Axelrod 1986). Also, in a representative democracy, even if norms of citizenship weaken, norms of what is appropriate behavior for legislators, administrators, or jurists may remain strong. For such individuals, knowledgeable pursuit of the public good remains the norm even if only observed in the breach.

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REVIEW ESSAY

IF INSTITUTIONS HAVE CONSEQUENCES, WHY DON'T WE HEAR ABOUT THEM FROM MORAL PHILOSOPHERS?

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Morality: A New Justification of the Moral Rules. By Bernard Gert. New York: Oxford University Press, 1988. 306p. $32.50.

The Theory and Practice of Autonomy. By Gerald Dworkin. Cambridg: Cambridge University Press, 1988. 166p. $34.50 cloth, $11.95 paper.

The Common Life: Ambiguity, Agreement, and the Structure of Morals. By Burton Zwieback. Philadelphia: Temple University Press, 1988. 211p. $24.95.

Morality within the Limits of Reason. By Russell Hardin. Chicago: University of Chicago Press, 1988. 207p. $24.95.

Social studies are moral studies. Ways of life (cultures) are normative constructs that enable people to answer such basic questions as, For what and to whom am I responsible? and How do I hold others accountable? Choices among ways of life—the institutionalized embodiment of rules specifying how individuals ought to live with each other—are moral choices. Thus, moral rules do not exist only on pages where anyone can write what they please; rules for decision are part and parcel of institutions that guide and constrain behavior.

Reading from the many works that constitute the renaissance of modern moral theory leaves me ambivalent—impressed with their high intellectual content, dissatisfied with their aura of unreality. It is not that these works lack examples or applications; on the contrary, they are imbued with the passions of the day. After mulling the matter over, I have traced my uneasiness to their slighting of institutional context, as if the values debated could be detached from the ways of life in which they were imbedded.

Each of the books under review has substantial merit. A great deal may be learned from them. Intelligently conceived and powerfully argued, these books can be taken to exemplify the best understandings available. Their admirable features make it possible to ask about the effects on moral consciousness of slighting the institutional context in which human life is lived.

Gert on Universal Moral Rules

Disparaging "the jargon that generally passes for moral philosophy," desiring "to make of morality a science" (p. xix), convinced he alone has the truth, the attention-demanding posture of Bernard Gert's *Morality* is integral to his approach, which requires readers to buy into his definitions. He begins well. Just as Karl Popper refers to refutation rather than proof as the essence of scientific activity and as students of democracy often stress the importance of getting governments out of office, so Gert makes avoiding evil (as opposed to achieving good) the centerpiece of his defense of traditional morality (omitting, to be sure, its religious sources of support).

"Insofar as the reader does not agree that everything I call irrational is irrational," Gert fairly warns his readers, "he will not agree with the conclusions that I reach" (p. xvi). Because he argues that one ought never to act irrationally, whatever is deemed irrational has the force of a negative commandment. Thus, he excoriates Hume for employing a false, procedural—instead of a true, substantive—definition of rationality. For if every desire were reasonable, evil desires could be called rational. Instead, Gert defines evil as "the object of an irrational desire." These evils, which he claims "everyone agrees" are evils, include "death, pain, disability, loss of freedom, and loss of pleasure" (p. 48). Gert's justification is that people would not rationally wish to suffer such evils; and though they might wish to inflict them, if they did so, others would be encouraged to do the same, thus increasing the probability that the original inflictors or those they cared about would suffer the same evils.

The arbiter of this ingenious web of definitions (so tightly argued it is more difficult to unravel than might be imagined) is a construct called "all impartial rational persons" (p. 10) who act within a public system. Impartiality toward a group, according to Gert, means that one's decisions are not influenced by whether a group is helped or harmed. And a public system is "a system of conduct that is both known and understood by all those to whom it applies [this excludes religion and most science] and one that it would not be irrational for . . . [them] to adopt to govern their own behavior" (p. 5). Since it would by stipulation be irrational for anyone publicly to advocate causing the evils Gert wishes to prohibit, he has, in his own view, erected a self-reinforcing system of moral rules from which there is no escape.

Gert concludes, "I have shown what morality is. Others are needed to teach people to follow it" (p. 303). Is that all there is to it? Although Gert clearly states that his moral rules do not presuppose "some more simple imaginary world" (p. 201), he does not ask why what he wishes occurs so infrequently. Why are humans so often irrational in his terms?

When an analyst keeps finding irrational patterns of behavior, something is wrong either with the analyst or with the subjects. I prefer to conclude that my understanding is at fault, for the subjects know their situation much better than an observer could.[1] Consider Gert's comment, "To the deprived citizens of a country it is clear that other persons do not care for them" (p. 303). No it isn't, for it is a staple of social science that the normative is what counts. Deprived adherents of a hierarchical way of life, for example, who legitimate large differences in resources, are unlikely to think of themselves as deprived; whereas those committed to egalitarianism are likely to perceive much smaller differences as immoral.

Part of the charm of *Morality: A New Justification* is the author's conviction that "all impartial rational people" attribute similar meanings to the same events. This is why he is able to say that "it is disagreement on the facts, or on their significance that is the source of almost all that is usually regarded as moral disagreement" (p. x). Leaving aside Quines's well-accepted position in the philosophy of science that all facts are theory-laden, endless disputes about the morality of public policies, from homelessness to nuclear energy to genetically engineered organisms, are characterized by disagreement both on facts and on the values that ought to apply. Without an agreed frame of reference—a frame that must be partial, not universal (or it would cease to select certain bits of evidence and select out all the rest)—disagreement is inevitable. To say, as Gert does, that "if, as seems plausible, all impartial rational persons would expect that people [should] give up at least some luxuries to provide others with necessities, then our moral standards will be much higher than they presently seem to be" (p. 212) is exactly to judge the question of which way of life ought to take precedence. In an inclusive hierarchy, where the sacrifice of the individual parts for the collective whole is the norm, for instance, Gert's proposition would apply. Egalitarians, however, would demand a great deal more redistribution, while individualists, apart from questioning how "necessities" other than biological survival are determined, would consider such state coercion to be grossly immoral.

It is true, as Gert contends, that "unless nonmarital sexual relations between consenting adults causes harm to someone, no impartial rational person would favor a public rule prohibiting such activity" (p. 135)? Individualists, who believe in self-regulation, would agree; but hierarchists, whose way of life depends on adherence to graded inequalities, would condemn the notion. To believers in hierarchy, Gert's error is clear: no social distinctions, no moral distinctions, no morality.

Separating moral rules from the institutional means for their fulfillment is a mistake. For one thing, we cannot understand morality unless we pay attention to why people want the different things they do in the first place. For another, we cannot alter or maintain moral standards unless we understand the institutional matrix in which they are shaped.

Dworkin on Autonomy

Gerald Dworkin properly views his account of autonomy not "as replacing moral and political conflict, but as providing a clearer picture as to what the conflicts are about, and indicating where those who accept such a theory take their stand, and the ideal of a person and society they advocate" (p. 165). In this, his helpful and discerning account succeeds admirably. In addition to the usual criteria of logical consistency, empirical possibility, and normative relevance, however, Dworkin wants his concept of autonomy to be ideologically neutral, so that, for example, "it should not be the case that only individualistic ideologies can value autonomy" (p. 8). Here he strives mightily but with only partial success.

After demonstrating that it is taken to be a central concept by moral theorists of diverse persuasions, Dworkin comes to autonomy as "a notion of the self which is to be respected, left unmanipulated" (p. 11). But how, he asks, given the centrality of social ties, is this possible?

Dworkin rejects a strong definition of autonomy as "the uninfluenced influencer" (p. 12) because it is "inconsistent with loyalty, objectivity, commitment, benevolence, and love" (p. 21). Instead, he conceives of autonomy, in a more interesting way, as "the capacity to raise the question of whether I will identify with or reject the reasons for which I now act" (p. 15); that is, autonomy is conceived of as "a second-order capacity of persons to reflect critically upon their first-order preferences and . . . to change these in light of higher-order preferences" (p. 20). Such critical reflection is indeed compatible with change from one way of life to another. But is it equally accepting of all the ways that are practiced? Apparently not, for Dworkin sees his conception as antithetical to "the uncritical acceptance of authority" (p. 29). By ruling out hierarchy, the type of social organization most people have lived in, so far as we know, since recorded history began, Dworkin loses his institutional impartiality.

Perhaps we might restore impartiality by considering how people adhering to a variety of ways of life would conceive of Dworkin's autonomy. Let us begin with Jürgen Habermas' (1975, 1979) celebrated "ideal speech situation," in which decisions are legitimated when they are a product of full discussion among people who have similar resources, including equal access to information. This excludes fatalists (who do not believe their participation matters), hierarchists (who would not want to engage in decisions above or even outside their station), and individualists (who will participate only if it is worthwhile to them personally and who, in any event, would not consent to leave behind the resources they have accumulated). This version of autonomy is suitable only for egalitarians.

In the spontaneous market order that Freidrich Hayek (1960, 1976) celebrates, the candidate for the autonomous individual would undoubtedly be the entrepreneur, whose risk-taking decisions benefit others by uncovering hitherto unsuspectedly advantageous combinations of resources. Evidently, those who detest risks and try to avoid them (fatalists), those who eliminate risks by governmental fiat (hierarchists), and those who claim that risk-taking competition harms others (egalitarians) are not, in this conception, worthy of being given the gold star of the autonomous personality. In Russell Hardin's perceptive formulation, "Hayek . . . inverts the message of Hofmannsthal's lines:

> Many naturally must die below,
> Where the heavy oars of ships strain,
> Others live at the helm above,
> Know the flight of birds and the lands of the stars.

In Hayek's view, many must have the luxury of life at the helm if others are ever to rise above the status of galley slaves" (p. 129).

Fatalists, who believe they can do nothing to affect their future, either deny themselves—or are denied by others—the status of being autonomous. Fatalists also deny others the possibility of being autonomous; for if nature is essentially random, the people who give them orders only fool themselves into thinking they can control their destiny.

From the hierarchical standpoint, as the work of Louis Dumont (1980) shows, individuals lack not so much autonomy as existence, for they are socially constituted. Both egalitarianism's lack of differentiation and individualism's lack of insulation are conceived as chaos. Adherents of hierarchy, then, can never be autonomous. Because the individual exists to serve the collective—indeed has no existence apart from it—only hierarchical institutions themselves can be said to be autonomous.

Each way of life, we see, denies autonomy to the others. Only fatalists are consistent in denying it to themselves as well. What about those who value autonomy as a good *in itself?* Anthropologist Michael Thompson (1982) has discovered an autonomous way of life whose features illuminate the obstacles in the way of widespread imitation of this cultural type. Autonomous people wish neither to dominate nor be dominated by others. This requires that they be neither fatalists who are dominated, nor hierarchists who dominate others. The autonomous culture is made up of people who, desiring neither to manipulate nor be manipulated, seek to get around involvement in the active ways of life. They may be thought of as being like individualists who, having attained sufficiency, refuse to build networks or egalitarians who, working just enough to get by, use the rest of their time to help others, without insisting (through government or other coercive means) that people not so motivated do the same. Under these conditions, the proportion of autonomous individuals must be fairly small. The odd-jobber, the watchman in big buildings, the subsistence farmer, are a few of only a small number of occupations that might make autonomy possible.

"An important motivation for moral discussion," Dworkin rightly says, "arises from our desire to justify what we propose to do, or have done, to others" (p. 59). Autonomous people (we call them hermits; see Thompson, Ellis, and

Wildavsky n.d.) do not *have* to justify themselves to others. Hermits are interesting and perhaps even valuable people, but since they escape from commitments undertaken by most other people, autonomy may not be the great moral achievement some philosophers currently take it to be.

Zwieback on Equality

Burton Zwieback admirably stresses the ambiguity of action. We understand our intentions not only prospectively but also by observing what they have become and retrospectively rationalizing them in terms of what we would like them to be:[2] "We engage in a process of constructing ideals and characterizations and then retroactively refining and reshaping them so that they fit together" (p. 51).

As he spells out the moral rules required to support his vision of the good life, Zwieback becomes increasingly certain that substantive equality is not only the only rational way but, more important, the right way of life. The merit of his book is its open avowal and principled argument in favor of greater equalization of resources as the only basis for legitimating collective decisions. Hence, his position is well worth understanding.

I was taught that democracy was possible because citizens legitimate the procedures for making decisions and do not, therefore, have to agree on the substance of policy. Not so, Zwieback argues, for they must believe that the outcomes will be broadly beneficial or they would not go along. "Now this condition," he tells us, "cannot be met where a substantial number of people face serious poverty, alienating social environments, radically constricted opportunities, and so forth" (p. 62). They need transfer payments to even things up. "Rousseau understood this perfectly," he continues. *"Equality is a prerequisite, not a consequence, of the common good"* (p. 63).

Although Zwieback's position does not require equality of result, "It does, however, envision an equality of condition—a situation in which the external conditions of life that bear, as far as we know, on our capacities for moral action will be sufficiently similar so as not to give rise to systematic advantages or disadvantages" (p. 158). Presumably, new inequalities resulting from past actions would not be allowed to influence future choices.

When I ask people whether they believe in equality of condition, I am often met with a disclaimer, similar to Zwieback's, that no sane person would ever approve so idiotic and morally reprehensible a concept of diversity "as identity of provisions, status, or power" (p. 167). Yet when faced with any of the usual distributions of provision, status, and power, I find they wish to diminish these differences. Let Zwieback sum up for himself:

> What this comes down to is the demand that egalitarians have made for generations: insofar as people's capacities to act are dependent on external conditions, those conditions must be equalized. This requires two things. First, it requires a serious narrowing of the range of incomes and wealth so that, while greater incomes and wealth—reflecting a need for incentives, rewards, and other market features—will be available, they will not be able to buy disproportionate advantages or create disproportionate disadvantages. Second, the conditions in which people live, work, learn, and participate must be sufficiently equal so that they do not generate significant advantages or disadvantages in respect of action or affect in any systematic way people's capacities for action. For example, we should have to create housing that is equally healthful and decent, if not equally luxurious; structure health care so that, when they are ill, people enjoy the same hospital arrangements, see the same doctors, have access to the same treatments; eliminate privileged schooling, so that children attend the same types of schools; and provide adequate social services, including means of remediation for those disadvantaged by circumstances (pp. 159-60).

Here is the explicit criterion against which many critiques of democratic capitalistic regimes are launched.

My objection to Zwieback's excellent book is one I would level at all those who write about morality without asking what is known about how people, organized according to their dominant values and social relations, actually behave. (If how people are organized does not matter, why do we bother to study politics?) Zwieback's explication of the social context forming egalitarian communities—decisions are binding on group members but all choices must be freely and explicitly arrived at—is similar to Mary Douglas's (1982, 183–254) strong group boundaries combined with a weak prescriptive grid. What is missing is institutional history.

Is egalitarianism a wonderful idea, hampered, alas, by human imperfection (including the regretable need for material incentives), or, like all other ways of life, does it contain unfortunate tendencies? Because egalitarians believe that human nature is good except when corrupted by wicked institutions, they find it hard to accept, for instance, internal conflict. It is, they say to one another, the product of evil outside forces (political radishes and policy oreos), secretly penetrating their virtuous collective. Hence, egalitarians drive conflict underground, the stories they tell about the virtue of flat-out confrontation being reserved for the established authorities that need to be chastened. Because it is difficult to resolve disputes without authority (Freeman 1983), and because authority is a prima facie case of inequality, their meetings take forever and their organizations are subject to perennial splits. In order to retain their membership, therefore, egalitarians are prone to exaggerate greatly the faults of the establishment.[3] Egalitarians also have difficulty accepting even favorable compromises lest they weaken the wall of virtue dividing them from the corrupt, that is, inegalitarian, world.[4]

If we go on to ask what happens when egalitarians take power, still more sobering observations may be made. For the two most recent instances, the cultural revolution in China and the government of the Cambodian Khmer Rouge, were without doubt examples of the attempt to enact radical egalitarianism, with theory and practice combined as closely as one can ever expect.[5]

Had I thought we were in as much need of being reminded about the ravages wrought by exclusive hierarchies, I could have referred to Nazism and Stalinism. And were I inclined to think more kindly of individualism, I might recall the distinction made by anthropologists who study New Guinea between "the big men" and "the rubbish men" (Pospisil 1963). The point is not that one way of life is wonderful and the others are terrible but that every way of organizing social life has grave potential for abuse.

Russell Hardin on Utilitarianism

Russell Hardin provides a half-way house between the discourse that now exists (morality as principles, precepts, rules) and what I would like that conversation to be about (morality as integral to ways of life). Because effective action usually requires institutional support, Hardin bemoans the separation of moral from political theory. To the usual plea for a more persuasive moral theory, he adds a plea for attention to "the ways in which our tastes, preferences, and other values are formed" (p. 207).

The most attractive features of Hardin's elegant and lucid exposition to laypersons (i.e., to all of us, outside our specializations) is his straightforward presentation, including, thoughtfully, definitions of key terms omitted by philosophers who apparently write only for those already in the know. Thus, Hardin's readers will come to know such important things as that value theory is akin to a theory of what constitutes and contributes to general *welfare* and that it is acceptable to speak of human *welfare* rather than just *utility*. (This lay reader appreciates being warned not to take the philosophers specializing in quick and dirty dismissal of an entire school of thought seriously. Is utilitarianism irrational because the time for action is over before the calculation can be complete? Is utilitarianism self-contradictory because it allows us to break promises for the greater good even though breaking promises is immoral? Is utilitarianism false because it favors the greatest good for the greatest number and everybody knows there is no greatest number? [Dworkin 1982]).

At last social scientists are on familiar ground. For Hardin makes his contribution by demonstrating that two things we know something about—cognitive limits on rationality and social interaction as modeled in the theory of games—are a great help in reaching conclusions about morality. How can we calculate the good (or evil) from our actions if each of us lacks the requisite information, if causal theories (or production functions) relating means to ends are absent, and if putting it all together in a single mind is beyond our capacities? You guessed it, the next thing that enters the picture are variations on Lindblom's theme of aids to calculation, especially incrementalism. There we are, like the man who spoke prose, doing moral theory without quite knowing it.[6]

Utilitarianism gets its moral impulse from its abiding concern with the consequences of acts for the welfare of others. Motivating people to do good by bringing about the right consequences is its chief rationale. Hence, Hardin would agree, I think, that it would not necessarily be rational for an agency to cut its budget to reduce a deficit unless other agencies did the same; for then the loss of welfare suffered by those served by the cutting agency might well be greater than the virtually nonexistent gain in deficit reduction. Hardin's "institutional utilitarianism" (p. 79) brings to mind the ever-influential language of the *Federalist Papers* as he refers to "institutions that will produce utilitarian outcomes from individually self-interested motivations" (p. 115).

An emphasis on institutions goes a long way with me, but not quite far enough. In justifying institutional utilitarianism, Hardin refers to Olson's well-known logic of collective action, in which the existence of individual motivations is not "consistent with collective interest or will" (p. 156). If free-riders are randomly scattered around societies, there is not much to be said about them except that they are there. But if small interest members with equal votes exploit large interest members in certain kinds off institutions more than in others, a lot can be said. The problem of free-riding, I claim (Wildavsky n.d.), is specialized to egalitarian institutions. Hierarchies seek to lessen its impact by socializing each individual to contribute to the whole collective. Individualists pay for participation. Fatalists do what they are told. Only among egalitarians is an equal voice on each issue required without regard to individual contributions and without legitimating coercion.

If what matters most to people is how they live with others and how others live with them, institutions ought not to be divorced from morality any more than preferences ought to be considered apart from why people have them (see Wildavsky 1987). Institutions have such profound consequences that no study of moral philosophy can be complete without inquiring into them.

Notes

1. See my analysis of why administrators in Nepal kept spending far less than their budget allotments (Wildavsky 1972).

2. Weick (1979) has made retrospective rationalization a staple of his analysis of organizations.

3. For a further analysis, see Douglas and Wildavsky 1982. For examples, see Berlin 1979, Butler 1974, and Embley 1967.

4. The history of the Student Non-Violent Coordinating Committee is useful in this respect. See Stoper 1983.

5. On the Khmer Rouge, see Feher 1983 and Jackson n.d. On the cultural revolution, see Dittmer 1987, Lee 1978, and Pye 1988.

6. I agree with Hardin that reports of the demise of utilitarianism are exaggerated. I also support his supposition that its decline in popularity is due to the difficulty of squaring it with notions of distributive justice (i.e., egalitarianism). Cf. Williams 1973 with John Harsanyi's life work, esp. Harsanyi 1976.

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Forthcoming in March

The following articles, research notes, and controversy have been tentatively scheduled for publication in the March 1990 issue:

David Austen-Smith and William H. Riker. "Asymmetric Information and the Coherence of Legislation." A Research Note.

William T. Bianco and Robert H. Bates. "Cooperation by Design: Leadership, Structure, and Collective Dilemmas."

Timothy Bledsoe and Mary Herring. "Victims of Circumstance: Women in Pursuit of Political Office." A Research Note.

James F. Bohman. "Communication, Ideology, and Democratic Theory."

Ian Budge and Richard I. Hofferbert. "Mandates and Policy Outputs: U.S. Party Platforms and Federal Expenditures, 1948-85."

Dennis Epple and Joseph B. Kadane. "Sequential Voting with Endogenous Voter Forecasts."

John P. Frendreis, James L. Gibson, and Laura L. Vertz. "The Electoral Relevance of Local Party Organizations." A Research Note.

Amihai Glazer. "The Strategy of Candidate Ambiguity." A Research Note.

Robert Grafstein. "Missing the Archimedean Point: Liberalism's Institutional Presuppositions."

Wendy L. Hansen. "The International Trade Commission and the Politics of Protectionism."

Keith Krehbiel. "Are Congressional Committees Composed of Preference Outliers?"

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Lucian W. Pye. "Political Science and the Crisis of Authoritarianism: The Challenge of a New 'Great Transformation.' " APSA Presidential Address.

G. Hossein Razi. "Legitimacy, Religion, and Nationalism in the Middle East."

Randolph M. Siverson and Harvey Starr. "Opportunity, Willingness, and Diffusion of War, 1816-1965."

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POLITICAL THEORY

Manhood and Politics: A Feminist Reading in Political Theory. By Wendy Brown. Totowa, NJ: Rowman & Littlefield, 1988. 256p. $32.50 cloth, $14.95 paper.

For Wendy Brown "*Politics* has been more exclusively limited to men than any other realm of endeavor and has been more intensely, self-consciously masculine than most other social practices" (p. 4). Through the writings of Aristotle, Machiavelli, and Max Weber, Brown elicits the relationship between manhood and politics in political theory. Approaches chronicling past theorists' justifications for the exclusion of women or highlighting realms of work and activity traditionally excluded from political life do not "substantively challenge the characteristics of political life itself" (p. 12). This challenge is the subject of Brown's book.

In her view, Aristotle, Machiavelli, and Weber all espouse conceptions of politics that require the repudiation or transcendence of nature, of the body, and of the realm of necessity lest these contaminate the political realm. Aristotle claims that man's *nature* is political. Yet the polis is a product of *convention,* and feeds upon slaves, women, and artisans—"the activities that initiate, constitute, and sustain his existence" (p. 37). Even the celebration of the body as an artifact of beauty or power subordinates necessity as the individual imposes "the 'form' of the most rational part of his soul upon the 'matter' of his body," thus "wielding . . . the body as an instrument of the mind" (p. 58). Superiority as the defeat of rival claims is thus central to both manhood and politics for Aristotle, as alienated men seek honor to convince themselves of their own worth, of their actions' immortality, and even of the reality of their own existences. Similarly, for Machiavelli, politics requires the imposition of form upon matter as *virtù* channels ambition to dominate or defeat the vagaries of *fortuna.* "Politics only appears when there is something to conquer, overcome, ambush, or resist" (p. 115), even if necessity must be artificially created to elicit citizens' *virtù.* But for Brown, irony appears in the identification of politics with control: "Independence is the goal and articulation of power, yet power is generated through interdependent relations, hence subverting itself" (p. 107). Finally, Weber strives "for a political *aesthetic* of power, prestige, national glory, and heroic leadership" (p. 141), which is distanced "from any substantive concern for the just ordering or well-being of the political body" (p. 144). But the capitalist economy and bureaucratic state, which began as means to state power, become ends; bureaucracy "comes to predominate *as* state power" (p. 157). Thus, Weber looks to the political or charismatic hero to recreate the virtues of political action. Charismatic leadership requires indifference to the routine demands of everyday life and devotion to heroism and to that which is extraordinary. Yet again, even "the politician's devotion to the state is devotion to an instrument that has become an end" (p. 172).

Brown devotes her final two chapters to visions of a "postmasculinist" politics: "Willingness to risk death is the proof that life has been discarded as a fundamental value" (p. 182). Even the welfare state invokes "the language of heroism" and "the sacrifice of life for empty causes" (p. 184). Brown is careful, however, to avoid easy solutions. If Beauvoir and Firestone seek to transcend the body while identifying male values with human ones, others seek simply to substitute feminine values for masculine ones in the public sphere, in Brown's eyes a questionable move when all gender is culturally constructed. She does not glorify nature, but suggests that "we need to find our freedom in and through relationships to necessity, by constructing free conditions through which to engage *with* necessity rather than by striving to overcome nature and necessity" (p. 196, emphasis added). Brown briefly sketches alternative approaches to freedom that involve struggle and conflict, but "not . . . to master or control necessity. Rather, it is a struggle to draw forth possibilities from necessity and to transform both ourselves and the necessary as we struggle with it" (p. 209).

This book is a provocative and important one, not only for its portrayal of the relationship between manhood and politics but also for its focus on what politics has included, rather than only upon who or what has been omitted. My major query centers upon her allusions to the common good. Brown suggests that the aesthetic of power contrasts with a politics whose guiding purpose is the common good (p. 141), ostensibly because of the separation of politics from necessity (p. 144). Yet according to exponents of the civic republican tradition, like Pocock, private ambition is distinguished from pursuit of the public good not by its objects but by the sensitivity to others' recognition that infuses the latter. Does Brown then define the common good solely by its subject matter, that of necessity? Or might attention to it be identified by the motivations of citizens and leaders, admittedly more difficult to pin down. If, on the other hand, an aesthetic of power precludes pursuit of the common good because its focus obscures class divisions, Brown should say so. Her analysis of manhood and politics describes no *necessary* contradiction between a masculinist politics and a pursuit of the common good, so it is unclear whether the good is conceived as something chosen by a political community. Otherwise, this book will reward not only the political philosopher, but also anyone interested in how we have become the "political animals" Aristotle claims we are.

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Political Theory and Modernity. By William E. Connolly. New York: Basil Blackwell, 1988. 196p. $29.95.

In the last several years "modernity" has made its way out of continental European thought, where it has resided fitfully for generations, and into our generous U.S. consciousness. *Political Theory and Modernity* is an attempt to make sense of this late migration, at least for teachers of political theory in the United States, by attempting to join a version of what is claimed to be Nietzschean critique with textual discussions of Hobbes, Rousseau, Hegel, Marx, and (for an unexpected contrast) the Marquis de Sade. Political theory in U.S. universities has typically been a "reading" of historical texts, and the author stays with this reliable format, writing three chapters and two "interludes" on the above theorists, who are then sandwiched between some reflections on Nietzsche and modernity that "privilege a Nietzschean perspective" (p. 15), as he says. There is a great deal more political theory and its history than modernity in the text, but the project does suggest, nevertheless, that new forces are astir in the venerable halls of theory.

By *modernity* the author seems to have in mind something quite basic: the condition Nietzsche depicts in his famous "death of God" passages in *Die fröhliche Wissenschaft* and elsewhere. It is not the dynamics of what thinkers as disparate as Daniel Bell and Jürgen Habermas have called aesthetic, cultural, or social modernity that stands at the center of this discussion. Instead, the simple loss of faith, the rise of secular critique and "genealogy," and the threat of nihilism delineate the motivating concern. In the analyses of the major theorists, therefore, one is always faced with the question of "faith": Hobbes is said to possess a "faith in natural reason," Hegel a faith in the "ontology of Spirit," and Rousseau a faith in all sorts of things. The point of exposing such presuppositions seems to be found in Nietzsche's remark that modern "science" demands a faith, from which it can acquire "a direction, a meaning, a limit, a method, a *right* to exist." And now all such "faith" is unmasked as embodying "the spirit of nihilism," for according to our author it "demands a definitive standard" that can no longer be sustained in our hypercritical world (p. 137). Just how empty the old verities have become can be glimpsed, apparently, in the Marquis de Sade's pornographic abandon, where, as with Georges Bataille, political order is overwhelmed by negative aesthetics.

Now all of this might be unexceptional were it not for the fact that Connolly seems to think of his forays into the canon of political theory as new, or at least as an attempt "to foment thought about the character and prospects of late modernity" (pp. ix–x). (I say "seems" because the hesitant prose of *maybe* and *perhaps* intrudes in numerous places, most alarmingly in the concluding appeal for a

Nietzschean "radicalized liberalism.") Does he succeed in this purpose? There are good reasons for doubt. "Modernity" is a more formidable opponent for "political theory" in its historical (and traditional) mode than these essays care to admit. Nietzsche's scorn for predecessors is not shared by Connolly, who tends instead to write within the linguistic world of his theoretical subjects, with only a sprinkling of appropriate avant garde terminology (*frame, sign, unravel*). What is striking about the discussions of Hobbes, Hegel, and Rousseau is precisely their progression through a standard range of interpretive questions familiar to teachers of the history of theory. The questions are usually answered, to be sure, but without *any* critical literature for establishing context and significance and with only the vaguest promise of benefits to come for understanding "late modernity." (The brief bibliographic appendix, pp. 183–89, proves an inadequate substitute). The result is both a catalogue of unchallenged assertions (e.g., "To become a Hobbesian individual one must give up much of one's own individuality," p. 32) and a strange and uncomfortable disjunction between "political theory" and "modernity."

So what has *Political Theory and Modernity* accomplished? It has shown, once again, the power of political theory's—and the the U.S. Left's—fascination with Nietzsche. I have no quarrel with fascination. But if Nietzschean critique were taken with the thoroughgoing radicalism it demands, it would lead not merely to commentary on canonical literature but to a transformation of the theoretical field and vocation. Connolly should have taken more liberty with his Nietzschean privileges. Had he done so, he would have seen that "reading" requires an entirely different discipline than that attempted in these pages.

LAWRENCE A. SCAFF

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Democracy, Liberty, and Equality. By Robert A. Dahl. New York: Oxford University Press, 1988. 286p. $29.95.

The publication of these ten essays by Robert Dahl, dealing with the broad themes of democracy, liberty, and equality, with an introduction placing them in chronological and substantive context, is the closest thing we have to an intellectual autobiography of our most creative political theorist. It is not until p. 22 that he tells us about the circumstances that attended the writing of his doctoral dissertation, from which the first three selections in this book were taken—"On The Theory of Democratic Socialism," "Marxism and Free Parties," and "Workers' Control of Industry and the British Labor Party."

Dahl wrote his dissertation under the supervision of Francis Coker during the nine months between September 1939 and May 1940. "During the time I was writing," he recalls, "it was impossible to predict except on blind faith, whether democracies would survive, and if so where and in what form, and what economic orders would emerge out of war, victory, and defeat. I do not much doubt that these times greatly strengthened my belief that the nature of the economic order—whether capitalist, socialist or whatever—ought to be subordinate to the primary values of democracy, collective self-determination, democratic liberties, and the equal intrinsic worth of all human beings" (p. 22).

Thus, from the very beginning of his career, Dahl has been preoccupied with the problem of reconciling liberty and equality within the framework and process of democracy. A whole generation of young political scientists were imprinted by these tragic years, but only he has continued to pursue the issues and problems in all of their logical and empirical ramifications.

In his doctoral dissertation, which he refers to as a "daringly ambitious effort," he analyzed "all of the major formulations of socialist ideas from the Utopians onward, evaluating each according to its likely success or failure in solving" the six problems that Dahl argued would have to be solved in order to reconcile socialism with democracy (p. 23). These included the problems of recruiting experts capable of coordinating the economy, while at the same time limiting discretionary authority and rigid and oppressive bureaucracy, and protecting criticism and opposition. At the end of the New Deal years Dahl thought that in the United States "a pragmatic program might effect a reconciliation between democracy and social control of the economy. Such a program would more nearly answer the six basic problems than any other that has been suggested" (p. 23).

Forty-five years later in his *Preface to Economic Democracy* (1985) Dahl still confronts the serious shortfall in U.S. democracy, unwilling to settle for its uneasy compromise with corporate power and inequality. While in his doctoral dissertation he favored a New Deal macro solution, in his current view he recognizes the political and social costs and consequences of government control of the economy and proposes a micro, socialist solution, drawing on, among other things, the experience of the Mondragon system of cooperatives in Spain and the plywood cooperatives of the Pacific northwest.

While he is not sanguine, he lets no one off the hook when he concludes, "I cannot say whether a people so divided possesses the firmness of purpose and the clarity of vision to assert the priority of democracy, political equality, and the political rights necessary to self-government over established property rights, economic inequality, and undemocratic authority within corporate enterprises" (p. 163).

In between these two books stands the corpus of Dahl's contribution to political theory, his early confrontation with the contrast between populist and polyarchal democracy in *Preface to Democratic Theory* (1956), his breakthrough on the democratic policy process in *Who Governs: Democracy and Power in an American City* (1961), his two splendid books on oppositions, his definitive study, *Polyarchy: Participation and Opposition* (1971), his reasoned argument with the populists of the sixties in *After the Revolution: Authority in a Good Society* (1970), and his critique of corporate power and privilege in *Dilemmas of Pluralist Democracy: Autonomy versus Control* (1982). Many of these larger themes are treated in the chapters of the book in review.

The reader of *Democracy, Liberty, and Equality* will find in it much of the substance and flavor of Dahl's larger body of work. Anyone reading it will be tempted to turn to the major treatments of these themes. In addition one encounters revealing passages (found only here or in relatively obscure publications) such as his bold confrontation of constitutional reform, his uncompromising rejection of government by experts, and his expression of faith in the growing capacity of ordinary humans for enlightened understanding, all to be found in his "Impediments to Democracy." This passage needs to be reread in the age of methodological individualism.

GABRIEL A. ALMOND

Stanford University

The Origins of Democratic Thinking: The Invention of Politics in Classical Athens. By Cynthia Farrar. Cambridge: Cambridge University Press, 1988. 301p. $37.50.

This engrossing, valuable book provides an instructive account of the political thought of three major intellectual figures of fifth-century Greece: Protagoras, Thucydides, and Democritus. By imaginatively and systematically constructing their views out of the elusive or fragmentary form of their written remains, the author, a Yale- and Cambridge-educated classicist now politically active outside the academy, has excavated a body of ancient Greek political thinking that has been hidden by the imposing theoretical systems of Plato and Aristotle.

Farrar's archeological uncovering addresses political as well as scholarly concerns. Plato and Aristotle are typically viewed as the founders of political theory. Because they also were harsh but trenchant critics of democratic principles and practices, their dominance over the ancient Greek theoretical terrain has disturbed students of political theory and ancient Greece who harbor democratic sympathies. Where are the *democratic* political thinkers of ancient Greece? Was there no theoretical justification for democracy to come from the world's first democratic societies? Could it be that the origins of political theory were entirely antidemocratic and that theoretical inquiry necessarily promotes antidemocratic views? In despair over the apparent absence in ancient Greece of democratic theory amidst democratic practices (argued by A. H. M. Jones over 30 years ago in *Athenian Democracy and Its Critics* [1957]), many have answered the last question affirmatively. Farrar finds the despair unwarranted and, therefore, the answer misguided. To the contrary, she believes that Protagoras, Thucydides (whom she especially admires) and Democritus constitute a fertile field of democratic thinking, whose richness outstrips anything provided by liberal political thought. And part of her project is to encourage the cultivation of this ancient soil so that we moderns may become better at "thinking [and] living democracy."

Since none of these thinkers unequivocally endorses democratic political institutions, Farrar identifies the democratic elements in their views by exploring the constituents of democratic discourse. She initially discusses the political and ideological environment of fifth-century Athens, where she finds an harmonious and healthy accommodation of political order and personal autonomy that is conceptually and causally linked to that society's democratization (and opaquely to "the invention of politics"). By implication, the ideas of Protagoras, Thucydides, and Democritus exhibit particularly faithful reflections of this world's principal values of life. Protagoras emphasizes the value of practical reason rooted in experience and judgment, as constituted by individuals in reference to personal action and communities in reference to collective action. Thucydides understands the relationship of human nature to social power by reflectively, historically construing the political interaction of reason and desire, freedom and order. Democritus articulates a home for the individual amid the cosmos by preserving the autonomy of the former and the objectivity of the latter. Each explains ways of thinking and acting that nourish democratic forms of life.

No one has made more compelling arguments than Farrar for how these thinkers could be read so as to express such modes of thought and why they possess democratic value. But she does not allay traditional doubts about the questionable coherence of their thought, the unreliability of our sources for it, and the substantive limitations of these thinkers and of Greek democracy.

Because Protagoras was a citizen of Abdera and professionally dependent on his acceptance by prominent citizens of Athens, the genuine political beliefs of this Sophist are extremely difficult to discern. Moreover, direct evidence of Protagorean thought amounts to a few sentences. We have to glean Protagoras' beliefs mostly from Plato's dialogues, whose aim in part was to refute whatever credibility they had. The first problem does not trouble Farrar. She deals with the second by artfully teasing a coherent Protagorean theory out of the text of his opponent and dismissing Plato's criticisms as misguided. Her effort here is more ingenious than persuasive.

As Farrar's reading of Thucydides' *History of the Peloponnesian War* insightfully reveals, this exiled Athenian general blended his own ethical and political views into an account of the Peloponnesian War. He produced an extraordinary text of historical illumination and political education. And because of Thucydides' profound recognition of the great virtues and dangers of the Athenians' relatively democratic exercise of political power, Farrar rightly calls attention to the importance of his history for both understanding and leading democratic societies. But Thucydides primarily offers a prudential analysis of how and why the exercise of political power in ancient Greece succeeded and failed. Hobbes in the seventeenth century and Strauss in the twentieth have drawn very antidemocratic lessons from this account. And Farrar unfortunately does not directly consider their interpretations.

As for Democritus (also of Abdera, not Athens), his thought survives only in unassembled fragments. Farrar carefully assembles them in an appealing fashion. But this is entirely her doing, nor is she particularly concerned about the epistemological or political coherence of Democritean atomism or the author's severe sexism.

Finally, Farrar somewhat idealizes the world and ideas she represents. She minimizes the effects, on the thinkers and thinking she commends, of the close interconnections between the Athenians' democratic life and their empire and between the egalitarian, participatory, and solidaristic shape of the political realm in classical Athens and its concomitant exclusion of women, institution of slavery, and presence of many depoliticized foreign workers. But one should keep in mind that Farrar's principal aim is to illustrate the insufficiently recognized conceptual value of Protagoras, Thucydides, and Democritus for political, and especially democratic, thought.

Politically minded theorists, historians, and citizens of all persuasions would benefit from reading this book. They may well finish it still feeling the need for systematically critical theories of justice of the sort that Plato and Aristotle provide, but they also will be far better informed about major democratic intellectual resources these theorists buried and truly enlightened about ways of living democracy in ancient, and possibly modern, times.

JOHN R. WALLACH

Vassar College

Max Weber and Thomas Mann: Calling and the Shaping of the Self. By Harvey Goldman. Berkeley: University of California Press, 1988. 284p. $30.00.

That Thomas Mann was enthralled by Nietzsche is common knowledge in German studies. By contrast, Max Weber's descendants in social science have denied (when they have not wilfully suppressed) Weber's indebtedness to Nietzsche. Weber was of course an influential teacher, whose understanding of responsibility shaped several generations in social science. He taught them how to defend liberal democracy and the institutions of representative government against the Left and the Right. Because they did not care to study Nietzsche (then in bad odor as the philosophic father of Nazism), they rather invented a Weber who shared their revulsion at Nietzsche's will to power and their ignorance of his writings. This tradition was summed up in Reinhard Bendix's *Max Weber: An Intellectual Portrait* (1959). Since its appearance, Walter Kaufmann has rehabilitated Nietzsche's reputation (not always by honest means), Nietzsche has become a darling of the Left despite his politics, and liberals—grown decadent—no longer have the spirit to defend free institutions. This generational change is reflected oddly in Goldman's book. His treatment of the old may remind those who know Plato's *Republic* of Socrates' dictum that everyone over 10 must be exiled. While Weber's relation to Nietzsche is not the focus of Goldman's essay, it is its philosophic center; the older Weberians and their knowledge of Weber's politics are nowhere in evidence. Goldman's silence might well make the old guard nostalgic for Wolfgang Mommsen's open challenge, *Max Weber and German Politics* (1958), in retrospect a model of respectful scholarly dialogue.

Rather than ask the traditional question (What might Weber contribute to the enterprise of social science?) Goldman approaches Weber from the perspective of Thomas Mann and fin de siècle German thought. In this perspective art as the shaping of the self is the dominant concern. Goldman examines Weber's religious sociology, especially *The Protestant Ethic*, in light of this concern and finds there a Nietzschean quest for a new aristocracy. This dimension of Weber's work has yet to be articulated adequately, in part because Weber was unable or unwilling to speak frankly about it. "About great things one must be silent, or speak with greatness" (Nietzsche, *Will to Power*, pref., sec. #1). Unfortunately, neither the legitimate political questions this aristocratic assertion raises nor the moral concerns of Weberian democrats nor the considered views of earlier students of Weber are seriously addressed in Goldman's book. He makes no effort to square his Nietzschean Weber with the Weber who (as they thought) held Nietzsche at bay.

Goldman evidently wishes to liberate himself from the concerns of the Weberians, including Mommsen. His essay is intended to go to the heart of Weber's attempt to secure the foundations of social science. Ignoring those who claim that Weber's contribution on this score was methodological or epistemological, Goldman argues that the foundation Weber provided was a new notion of the self. The pathos of *The Protestant Ethic*, on Goldman's reading, is the suffering of imprisonment in the bourgeois self and of Weber's spiritual struggle to revolutionize it from within. He contends that Weber's work on economic ethics and religion should be understood as a heroic effort to escape from the bourgeois doctrine of the self. Ultimately, he finds, Weber failed to make the complete break that the new self required. Goldman is in good company in thinking that Weber's Kantian heritage held him back: his essay confirms Leo Strauss's suggestion on this point, while ignoring the doubts I raised in my study of Weber and Nietzsche, *Political Leadership and Nihilism* (1984).

The liberation at which Goldman aims is intelligible only to the extent that he knows what we are to be liberated from. Unfortunately, he merely presumes (as do most who professionally shock the bourgeois) that we adequately understand the character and origins of "the bourgeois self." He himself seems thrall to Weber's account of these origins. To be sure, Goldman is aware that Weber's thesis on the origins of the capitalist spirit has been discredited beyond rescue; and he prudently abjures every intention to defend the thesis. That Weber's account is a ghost story does not perturb Goldman because he evidently assumes that a verifiable history would corroborate Weber's key assertions regarding the ethical irrationality of the bourgeois self and the causal irrelevance of modern political philosophy in its emergence. Students of politics should not be so quick to sell reason short. For what is at stake is the possibility of understanding and governing ourselves. The stupefying premise of Goldman's work is that the main features of our social world are unintelligible and cannot be established by scholarship. That myth is taken on by Hiram Caton's magisterial study, *The Politics of Progress* (1988), which explains better than Goldman's study why the Weberian quest for self-knowledge has ended in obscurantism regarding the most obvious features of our polity.

ROBERT EDEN

Hillsdale College

The Materialist Conception of History. By Karl Kautsky. Abridged, annotated, and introduced by John Kautsky. Translated by Raymond Meyer and John Kautsky. New Haven: Yale University Press. 558p. $55.00.

Karl Kautsky was a central political and theoretical figure in the popularization of Marxism during the period extending from the early 1880s to World War I, the period of the Second International. He wrote *The Materialist Conception of History* during the five-year period ending in 1927, after his position of dominance in the socialist movement had ended. The book represents the culmination of Kautsky's theoretical life work. Its distinctive contribution is an attempt to read the Marxist conception of history in the key of Darwin's theory of evolution, especially as that theory had been developed by Lamarck. The present edition, exhaustively annotated and extensively abridged, is the first English translation of the book. The abridgement is extensive enough to leave unsatisfied readers who may have wanted to approach "the text" from a deconstructionist perspective. It does, however, have the effect of focusing the readers' attention on the core theoretical contribution of the book: the relation between Marx and Darwin. A relation between Marxism and Darwinism had originally been suggested by Marx himself when, in an 1869 letter to Engels, he had identified *The Origin of the Species* as "the book which contains the basis in natural history for our view" (quoted in S. J. Gould, *Ever since Darwin* [1977, p. 26]). Kautsky's *Materialist Conception* must stand as a significant text for modern scholars who may want to pursue the connection between Marx and Darwin; and, as I shall suggest below, there are good reasons for wanting to pursue the connection.

Readers may want to guard against a tendency to view Kautsky as a figure of the past and a corresponding temptation to treat *The Materialist Conception* exclusively as a text of "its times," with little relevance to current debates and theoretical movements. The editor's own introduction to the book, for example, overemphasizes the extent to which Kautsky, writing in the 1920s, was presenting scientific views and deploying intellectual attitudes that are now outdated. For others, the temptation to read "the text" in the key of "its times" may be reinforced by Kautsky's political persona (the politics of the Second and the Third Internationals and the fact that Kautsky himself used a few opportunities in the text to criticize the revolutionary practices and peace that he was seeing "forced" in the name of Marxism under Lenin). While it is obviously true that *The Materialist Conception* can be read as a document of its times, it is also possible to see in the book—in the imaginative use it makes of Darwin to read Marx—two contributions to the modern landscape of Marxian thought. First, the book contains many well-developed illustrative examples of the Marxist theory of history and can be used as a very good primer in Marxian theory. Second, and most importantly, the book can serve as a basis for a further development of Marxism, and this along lines that are strikingly modern, current.

From the standpoint of current Marxian theory, what is most significant about *The Materialist Conception* is that Kautsky presents history as a process that, just like Darwin's natural evolution, is nonpurposeful, nonteleological. This nonteleological construction of history happens to coincide with much of the work done in post-World War II Marxism. The criticism of essentialist and teleological readings of Marx and the development of a nonessentialist Marxism was the motivating force behind the work of the French Marxist philosopher Althusser and continues to be a distinguishing feature of a school of thought in the United States (e.g., Resnick and Wolff, *Knowledge and Class* [1987]). In its own way, much of the work that can be associated with the Frankfurt school can also be connected, at least potentially, with an attempt to build a nonteleological concept of history. The investigation of cultural processes—a distinguishing feature of the school—presupposes a nontransparent, and possibly nondeterministic, relation between economic and cultural and political processes. It thus makes it possible to conceive the process of historical change as nonteleological. In the context of this work, Kautsky's own construction of history as a nonteleological process can well be seen as having a modern ring.

There is an irony, which cannot be passed over in silence, in this association of Kautsky with current attempts within Marxism to reject economistic and teleological readings of Marx as vulgar. It has become commonplace among Marxist scholars to attribute these vulgar readings to the Second International and thus to Kautsky himself. It seems to me, however, that the appearance of *The Materialist Conception of History* renders necessary a rethinking of the question of the origins of economism and of teleology with Marxist theory. This would not be the first time that the appearance of a text from the past necessitates a reconstruction of intellectual history while at the same time suggesting ways of thinking for the present. To the extent that this will happen with Kautsky's text, it will be a book of *our* times as well as *its* times.

ANTONIO CALLARI

Franklin and Marshall College

Whose Justice? Which Rationality? By Alasdair MacIntyre. Notre Dame: University of Notre Dame Press, 1988. 410p. $22.95.

The tentative answer is "Aquinas'." Because conceptions of justice and rationality can be defended and elaborated only within a given tradition, MacIntyre argues, we must answer these questions. Further, since justice and rationality necessarily hang together, one's justice and rationality must derive from the same tradition. MacIntyre plumps for Aquinas' resilient rationality and comprehensive justice largely because of its theoretical recognition of the dialectical means necessary to defend moral claims within a tradition.

To show the interdependence of conceptions of justice and rationality and their mutual embeddedness in social circumstance, MacIntyre chronicles three traditions: the "Aristotelian" tradition founded by Socrates and culminating in Aquinas, the Augustinian tradition that united Stoic and Judaic roots and yielded both Thomistic and Calvinist branches, and the Scottish Calvinist tradition best represented by Hutcheson and undermined by Hume. While MacIntyre illuminatingly describes how rationality and justice are connected in each of these traditions, he falls far short of showing that the connections traced are necessary ones.

Although he is willing to recognize recent liberals such as Rawls and Rorty as self-consciously developing their liberalism within a tradition (p. 346), MacIntyre leaves its history to others. With rather less sympathy and nuance, he characterizes liberalism as reducing all reasons for action to mere individual preferences (pp. 336–40)—thus ignoring Kant and his followers, who are not seriously covered in the book. For MacIntyre, liberals still suffer from the big Enlightenment mistake, which was to discard dialectic in favor of foundationalism.

MacIntyre's brief history of philosophical dialectic valuably deepens our understanding of what is now a rather common antifoundationalism, even among liberals. Thus, while MacIntyre casts Aquinas' synthesis of the Augustinian and Aristotelian traditions as the premier exercise in dialectic, Rawls also traces his antifoundationalist dialectic back to Aristotle (*A Theory of Justice* [1971], 51n.). The great advantage of this form of dialectic is that it offers a discursive path *to* first principles rather than supposing that these must simply be "found" in common sense, pure reason, or fundamental laws of human belief. Going beyond run-of-the-mill antifoundationalism is MacIntyre's further claim that dialectic can show one tradition rationally superior to another despite their being conceptually incommensurable, hence posing difficulties of translation. How? Rather in the fashion of a new Kuhnian paradigm establishing itself, though MacIntyre does not himself make this comparison. On this point, MacIntyre may have justified complaints both with the philosophers who have presented conceptual arguments against the possibility of incommensurability as if they resolved all practical worries about intercultural communication and with Nietzschean and other perspectivalists, who take incommensurability to foreclose timeless truth. Unfortunately, he rebuffs Nietzsche's alleged "insults" on the basis of insults, not arguments (pp. 368–69) and remains superficial in his arguments against his more complacent opponents. For instance, MacIntyre here belittles the force of Donald Davidson's "On the Very Idea of a Conceptual Scheme" (*Proceedings of the American Philosophical Association* 47 [1974], which involved the (Quinean) claim that since the meanings of a language's terms are all interlinked, translational adjustments regarding other terms can always avert any localized untranslatability. Although MacIntyre's own later presidential address ("Relativism, Power, and Philosophy," in *Proceedings of the American Philosophical Association* 59 [1985]) addressed Davidson more forthrightly regarding truth—a streamlined version of these arguments appears in chap. 18—he nowhere blunts the attack on "conceptual schemes." Yet MacIntyre continues here to speak about them blithely, developing his suggestions about localized untranslatability in a way that implicitly denies the holism of meanings.

A striking example of this is MacIntyre's comparison of Hume and Aristotle on whether practical reason governs or serves the passions (p. 329). He insists that in this case, as elsewhere, "logical incompatiblity *and* incommensurability may both be present" (p. 351), so that, in particular, Aristotle and Hume may be taken to have differing views about one and the same question. Failing to take seriously even his own warnings about difficulties of translation, MacIntyre nowhere makes clear what Aristotelian term(s) he takes *reason* to be translating; nor does it seem that he could give any univocal answer to this question. Whether Aristotle and Hume differ on the relation of reason and the passions will depend crucially on how one decides to map such Aristotelian terms as *nous, logis, bouleusis,* and *dianoia praktikē* to Hume's vocabulary. It is precisely into this room for decision that the meaning-holist steps in order to undercut the coherence of an alleged incommensurability of conceptual schemes.

There is a lot of fascinating detail in *Whose Justice? Which Rationality?* Everyone will learn something from it, whether about the political roles of Socrates's interlocutors, Augustine's "invention" of the will, or the Scottish Enlightenment and Hume's apostasy from it. MacIntyre's storytelling mode is engaging but ultimately inconclusive as a general defense of dialectic, an account of rationality in the face of incommensurability, or a brief for the superiority of the Thomistic tradition. Perhaps MacIntyre would concede this charge of inconclusiveness, since it is implied by the "essential incompleteness" of dialectic (p. 172, cf. 364). If so, however, he should cease to brandish the inconclusiveness of contemporary debates about justice as if this were a special indictment of liberal modernity (p. 344).

HENRY S. RICHARDSON

Georgetown University

Theories of Civil Violence. By James B. Rule. Berkeley: University of California Press, 1988. 345p. $37.50.

James Rule's book delivers both less and more than its title promises. It does not present a conventional review of current theoretical works on civil violence. However, it does deliver a comprehensive, thoughtful, and challenging analysis of the paradigmatic roots of contemporary civil violence theory, which challenges the student of civil conflict to go beyond the normal scientific labors of model building and hypothesis testing and reflect on the validity of those "first principles" from which current theories are derived.

Rule has taken on an ambitious task: to analyze the major theories of the origins and characteristics of collective violence as a means of assessing the progress and prospects of social theory as a scientific enterprise. Relative deprivation, rational choice, resource mobilization, and other perspectives receive ample attention in this book. However, Rule is concerned not with their respective explanatory power but with whether these works, considered as a whole, constitute the sort of cumulative body of knowledge that we should expect as an indicator of the "scientificness" of research.

Rule approaches this ambitious venture by first proposing a set of standards by which to assess the scientific status of a body of research. If there is a weakness in this book, it is that the evaluative criteria he chooses are perhaps too pedestrian for the epistemological task he set for himself. For Rule, "falsifiability" becomes the ultimate yardstick for measuring the scientific import of theory. Rule's arguments would be more coherent, rigorous, and compelling had he developed a more deductively elegant set of instruments.

Rule's work goes beyond those works that comprise the literature on civil violence. His point of departure for each chapter is the paradigmatic ancestors of a particular school of civil violence theory. He begins with a consideration of Hobbes's works as the earliest and clearest articulation of the central question of civil violence research, the problem of order. What arrangements among self-interested individuals are necessary for civil order to prevail? How do we explain the transition from order to periods of violence and back again to conditions of relative peace? It is here that we first encounter the deficiency of his evaluative approach: Is it appropriate to assess the impact of Hobbes on modern social theory by asking only whether we can derive falsifiable propositions from his writings and confront them with appropriate data?

Rule is more effective and interesting when he is tracing derivation of specific civil violence research traditions from the broader social-scientific paradigms of the intellectual giants of social science. Thus, contemporary rational choice theorists such as Hardin, Rogowski, Denardo, and Popkin are traced to their Hobbesian origins. The works of Jeffrey Paige and Eric Wolf are assessed in the context of their derivation from Marx. Smelser, Kornhauser, and Huntington are discussed as descendants of Durkheim, Park, and Parsons. This analysis of the historical development of social science theory leads Rule to his most profound insights. For instance, in discussing Pareto as a reaction to Marx and Tilly as a reaction to Parsons he makes a compelling argument that paradigm shifts in the social sciences are often stimulated not by anomalous empirical findings but by changes in the societal values and political context in which social scientists labor. Thus, Pareto came to be seen as "a Marx for the middle classes"—who were disturbed by the political threat of Bolshevism. Similarly, Tilly's depiction of civil violence as rational action is seen as a reaction to Parsonian notions of value integration that depicted civil violence as irrational, deviant, anomic, or otherwise pathological. Rule argues that for Parsons and his generation, militant social action was associated with "Naziism and Stalinism abroad, McCarthyism and lynchings at home," whereas for the generation of scholars that emerged in the 1960s, civil violence was associated with "national liberation abroad and peace and civil rights demonstrations at home" (p. 289).

Because it is ambitious with respect to the questions it raises, this book does not always deliver answers that are completely satisfactory. But because it is ambitious in its challenge to the intellect, it is the sort of work that students of civil violence should periodically return to. Rule takes us beyond questions of data and methodology to considerations of the epistemological foundations of civil violence research and forces us to confront the question of just how scientific our endeavors are, a question that is all too easily avoided in the everyday work of social science research.

T. DAVID MASON

Mississippi State University

In Defence of Liberalism. By D. A. Lloyd Thomas. New York: Basil Blackwell, 1988. 133p. $39.95.

Only in his postscript does Thomas indicate why he thinks liberalism now requires defense. Echoing apprehensions voiced by John Stuart Mill in the opening pages of his essay on liberty, Thomas suggests that expansion of the West's capacity to furnish the conditions of material well-being has not been matched by a correlative increase in our commitment to the pursuit of intrinsically valuable goods and activities. Convinced that preoccupation "with improvements in the instrumental aspects of life" bolsters the appeal of "low-level leisure activities, such as TV soap operas and spectator sports" (p. 125), the author advances a justification of the liberties whose protection enables persons to "come to have more reasonable views about what is worthwhile" (p. 123).

Thomas argues that liberal political theory is defined by its effort to justify a neutral constitutional framework without, in so doing, privileging a specific conception of the good. Holding that Rawls's deontological formulation fails to meet this imperative, Thomas advances a consequentialist argument that he labels "eccentric" because, unlike classical utilitarianism, it does not aim to maximize what liberalism's agents already know to be instrumentally good. What those agents should know is that all share an interest in the institutional conditions that make it possible to search for whatever possesses intrinsic value even though no one can ever be unequivocally certain of the "truth" of his or her discoveries. Thus, on the basis of a premise not unlike Alasdair MacIntyre's avowedly nonliberal characterization of "the good life for man" as "the life spent in seeking for the good life of man," Thomas insists that "if people are to have well-founded views about what is of intrinsic value, it is necessary that they should have the right to experiment with different 'modes of existence' " (pp. 107–8).

The liberties whose protectection makes possible such experimentation are grounded in a view of the rights that persons are to be equally assigned in virtue of their contribution to liberalism's end. Including freedom of speech, publication, assembly, movement, life-style, and so on, these are derivative from the fundamental right of "self-ownership," that is, the right of "each person to control his or her own thoughts, talents, capacities, actions and body" (p. 8). The opportunity to exercise this right in a meaningful way, continues Thomas, presupposes the ability to exert some measure of control over the "material world external to their bodies" (p. 67); and so liberalism must acknowledge a right to private property. That right, however, is not unlimited; for if its legal protection engenders significant inequality, as it does within laissez-faire capitalism, some will be denied the resources needed to pursue what is intrinsically valuable. Hence, the right to property is to be qualified, where necessary, so as to accommodate the universal and prior claims of self-ownership.

This last conclusion leads Thomas to a broader question: What features of modern society most effectively discourage persons from experimentally exploring for what is nonrelationally valuable? Answering by pointing to an economic order that defines most occupations as mere means and that construes all goods as consumable commodities, he offers a criterion with which to judge the congruence of capitalist practice with the imperatives of liberal theory: "To what extent will a given system of rights of control have a long-run tendency to relieve people of 'economic necessity': to relieve them of engaging in activities they consider to be wholly of instrumental value, and to provide them with adequate resources for the pursuit of what they consider to be worth while in itself?" (p. 72).

Upon reading this work, one cannot help but wonder whether Thomas has saved liberalism from itself or merely regenerated the difficulties encountered by Mill in his struggle to temper the crassness of Benthamite utilitarianism. That he appears to have done the latter can be intimated by making two related points. First, committed to maximizing the freedom to pursue self-defined goods, Thomas refuses to offer a priori grounds for branding illegitimate any substantive conception of the worthy life. Yet he also acknowledges that liberal selves will abide others' experimentation with novel forms of living only if they possess certain specific virtues. A "tolerant temper," Thomas asserts, is not "an 'optional' moral requirement from a liberal point of view" (pp. 52–53); and so various forms of "paternalistic interference," including education "in those activities considered to be worth while in themselves (according to the cultural traditions of that society)" (pp. 55, 88), are unobjectionable. Whether such ethical cultivation coheres with liberalism's libertarian bent remains an open question.

Second, and more significantly, one must wonder about the adequacy of a theoretical endeavor that, troubled by the instrumentalization and commodification of all goods within a late capitalist political economy, responds by affirming the inviolable rights of self-ownership. As Thomas acknowledges, this notion of the self depends on an "analogy between the rights of control people have over their own persons and the rights of control owners of property have over what they own" (pp. 8–9). Just how completely this analogy has shaped his thinking is revealed, first, when he defines intrinsic value as an object's exclusive possession of a non-subject-dependent property (p. 101) and second, when he suggests that communitarian critics of liberalism who argue that personal identity is irreducibly constituted by membership within historically grounded collectivities presuppose "a labor-mixing argument for property rights" and, on that basis, defend "equal joint ownership by the living of all the living fellows" (p. 65). In light of this (mis)translation of the communitarian argument, we might well ask, Does not the analogy between the rights of self and property ownership encourage the sort of utilitarian objectification that transforms all intrinsic goods into means of market exchange? Will not the self, so defined, construe her talents as so many exclusive resources which, as disposable capital, await investment for the sake of future dividends? If so, this book's prescription may well aggravate the disease it hopes to cure.

TIMOTHY V. KAUFMAN-OSBORN

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AMERICAN POLITICS

Getting the Donkey out of the Ditch: The Democratic Party in Search of Itself. By Caroline Arden. New York: Greenwood, 1988. 178p. $39.95.

The Paradox of Mass Politics: Knowledge and Opinion in the American Electorate. By W. Russell Neuman. Cambridge: Harvard University Press, 1986. 241p. $29.95 cloth, $12.50 paper.

Concern over the gap between the norms of democratic theory and the realities of low public awareness of political affairs and political participation have been voiced for at least a half century. Early in the *Paradox of Mass Politics,* Neuman acknowledges that the questions posed by this gap are not new, going at least as far back as Walter Lippman's work on public opinion. The paradox of mass politics is that the political system works well despite its inability to more closely approximate democratic norms. As Neuman reminds us, studies of decision making in Washington also show that "an articulate voice of attentive public opinion is being heard" (p. 4).

In exploring the paradox he examines four theories that have attempted to resolve it and finds that all of them are "flawed" because none correlates political knowledge and opinion and behavior. To remedy the flaw, Neuman develops an index of "political sophistication," which he draws from nine voting studies conducted between 1948 and 1980. Neuman's three distinct but interrelated dimensions of political sophistication include political salience, political knowledge, and political conceptualization. He identifies a "sophistication continuum" along which three distinct "publics" can be located: 5% are politically active and informed; 20% of the public are apoliticals who neither participate nor pay attention to public affairs and do not share democratic norms regarding the importance of participation and awareness; and 75% are "marginally attentive" to political affairs but accept democratic norms of participation and awareness.

One of the significant contributions of this well-written and crisply argued study is its analysis of the relationships between levels of political sophistication and other variables. Surprisingly, he finds weak linkages between political sophistication and other important variables such as frequency of expression of political opinions, the structure of opinion, liberal-conservative ideology, and political tolerance. Accordingly, Neuman seriously questions the theory of stratified pluralism (among others), noting, for example, that "the notion of large scale crosscutting, issue-public pluralism may reflect the wishful thinking of analysts more than the behavior of the mass citizenship" (p. 73). Again, in examining the "basic premise . . . that political knowledge breeds moderation" (p. 158) Neuman finds only a weak and inconsistent relationship between authoritarianism and alienation and levels of sophistication and political participation.

Neuman attributes the ability of the system to work to the "pivotal role" of "an informally defined activist elite within the mass public" (p. 187), which subtly interacts with the inattentive mass. Although the mass public pays little attention to most "issues," it is nevertheless attentive to "fundamental issues" involving matters such as war and peace, economic health, and civil rights. This is not the stratified pluralism that reassured most scholars in previous decades. Rather, as Neuman notes, it is a model of "three publics" presumably held together by a small active elite and a periodically mobilized public.

Although Neuman's book is quite different in scope and intent from Arden's *Getting the Donkey out of the Ditch*, both books address in their own way the linkage between active elites and the less politically sophisticated and often inattentive mass public. Arden's sometimes repetitious chronicle of the Democratic party's search for meaning and mission after the "debacle of 1980" describes a defeated party badly in need of leadership but suffering on the one hand from too much democracy and on the other from an elected and organizational leadership often divided by the pursuit of self-interest. The book is a detailed case study of the Democrats' basically floundering efforts from late 1980 through 1982 to regain their footing. A very generalized assessment of the 1982–1987 period is offered in the concluding chapter.

The author's biases are clear. The party reforms that began in 1964 and ran through the mid-1970s were "on balance, . . . good, but like so many other progressive movements, they just did not know when to quit" (p. 13). Wider representation of women and minorities as well as other party groups that were reflected in delegate selection rules did make the Democrats a "national, mass party" by moving power away from state party leaders and toward a national organization and agenda. However, the quest to maintain balance and stability among diverse constituencies "embodies the Party's tragic flaw" because such often "frantic and random" searches "most often [*had*] led to the Party's disarray" (p. 16).

The disarray is abundantly illustrated in Arden's discussion of the Democratic party's search for a "new mission" and leadership after its loss of the presidency and its majority in the Senate. The largely unsuccessful struggles to build party leadership, restore organizational strength, and articulate a program were reflected in party efforts to give elected state and national officials greater voice in party decision making and a more effective role in the presidential nominating process; in the 1982 party conference; in the activities of "extralegal" policy groups formed by various active elites within the party; and within the congressional party structure. Arden is especially critical of the disarray that marked the search for program and leadership within Congress. House and Senate party leadership resisted attempts at policy development through the Democratic National Committee and engaged in rhetoric described as "self-serving grandstanding." Arden postulates that "parties under stress will become particularly active," a thesis illustrated by the behavior of congressional Democrats, who, however, were also bereft of ideas and "in doubt as to where they stood as a party on ideology" (p. 85).

Arden sees more hope for the party at the close of the decade. She concludes that there are signs that the party is pulling toward the center and becoming more concerned with ideological issues, a conclusion that many would debate from the hindsight of the 1988 campaign. It is clear that Arden wants a Democratic party with a more sharply defined mission, and she sees this as possible only if self-interest and the bitter debate that marked the first half of the decade are ended. The party needs to nurture "the central concerns of ideology and leadership" and must develop a "unified message" (p. 163).

Getting the Donkey out of the Ditch is very much the story of the struggle for control *among* competing elites in the party and in some ways the story of the struggle *between* the democratized party and its activist cadres. Arden does not make this point explicitly, but she does find that one of the "unintended consequences" of the redistribution of power within the party that resulted from the reforms of the 1960s and 1970s was "the loss of control by an elite corps of amateurs and professionals in steering the Party's direction" (p. 157). Neither does she address the common argument that post-1968 party reforms produced a Democratic party largely controlled by a liberal elite and substantially out of tune with some of its rank-and-file middle-class constituencies. At several places she does say that the move toward the political center is necessary to the electoral success of the party, and she also recognizes that "Americans will be attracted more to a good race at the convention and a good race in November than they will be in intellect and ideology" (p. 45).

Although these two books address quite different subject matters, it can be noted that both are concerned with the role of elites within the political system. Neuman disparages political campaigns as superficial and tends to blame the voter's lack of political sophistication for the condition. On the other hand, Arden's treatment of the problems of the Democratic party places criticism on party activists and elites for failing to present the voter with alternatives based on a clear sense of mission and purpose. If, as Neuman contends, the mass public is largely inattentive to other than broad and fundamental issues, a call for party leadership with clearly defined ideology should make little difference in restoring the Democratic party to former strength. Short of overarching issues that activate voter awareness, the party must still depend on organization, money, and perhaps emotional appeals for success in presidential elections. As for Congress, the incumbency factor, as Morris Fiorina and others have shown, will most probably continue to play the critical role in elections.

THOMAS D. UNGS

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The Republican Party in the U.S. Senate, 1974–1984: Party Change and Institutional Development. By Christopher J. Bailey. Manchester: Manchester University Press, 1988. 166p. $35.00.

The changing character of the Republican party in the 1974–84 decade, the change in the Senate during the same general period, and the impact of Republican control on the Senate in the early 1980s are all themes worthy of extensive research. Bailey attempts to combine the three in this book. He argues that "between 1974 and 1984 the Republican Party in the U.S. Senate was transformed by changes in the electoral environment. Not only did the increased use of political consultants, and the more active involvement of Political Action Committees in campaigns, weaken the ties between candidates and the party, but the internal distribution of power within the GOP was altered by the influx of Republican senators from previously Democratic states" (p. 129). The result was a change in political agenda and "an increase in individual assertiveness in the Senate, and the development of a new political style for the Republican party" (pp. 129–30). The Senate became "increasingly anarchic as individual senators asserted their own independence at the expense of collective action" (p. 12). The inexperience of many of the Republican senators thrust into positions of authority after 1980 further exacerbated the problem, with the result often being "legislative deadlock" (p. 12).

Perhaps because he tries to do too much, no aspect of his argument is analytically well developed or convincingly substantiated. Thus, the two facets of the hypothesis concerning the changes in the electoral environment should have been disentangled but are not; that is, the forces leading to candidates' independence of party and those that changed the regional and ideological composition of the Republican party are not identical and did not occur simultaneously. Most scholars would date the former considerably earlier than Bailey does. Yet he does not deal with that discrepancy. Also, he largely ignores the obvious fact that this change would influence Democrats as well as Republicans.

Documenting the change in the composition of the Republican Senate membership is a relatively tractable problem, and Bailey presents the appropriate data to show the change in regional composition. His analysis of the change in ideological composition is, however, deeply flawed by his not explaining how he arrived at his ideological categorization (see p. 65). There is throughout a casual attitude towards data and complete reticence about methodology. Bailey never tells us what parts of the study, if any, are based upon primary data, and often he does not systematically marshall data from secondary sources to substantiate his arguments.

His arguments about change in the Senate are no more analytically precise or carefully substantiated than those about the effects of electoral changes. There is a good deal of data available to show change—data on senators' increasing frequency of offering floor amendments as well as the increase in filibusters. Those data are not systematically used and, in any case, show that the changes Bailey dates to the late 1970s actually occurred earlier.

Bailey's problem is that he attempts to collapse two relatively distinct processes into one. The transformation of the Senate from the insulated men's club that Matthews describes to the individualistic Senate of today is simply not closely causally related to the processes that transformed the Republican party in the 1970s and early 1980s and those that resulted in Republican control of the Senate in 1981. To be sure, they cannot be treated in complete isolation from one another; but Bailey's failure at analytically disentangling them results in an analytically fuzzy argument not well substantiated by data.

BARBARA SINCLAIR

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Our Time Has Come: A Delegate's Diary of Jesse Jackson's 1984 Presidential Campaign. By Lucius J. Barker. Urbana: University of Illinois Press, 1988. 233p. $21.95.

To understand the presidential election of 1988, one must understand Jesse Jackson and his supporters. And to understand Jesse Jackson in 1988, one must understand Jesse Jackson in 1984. By this logic, *Our Time Has Come* is important reading for any serious student of presidential elections.

Lucius Barker helps us understand Jesse Jackson by giving us the chronicle of a black political scientist during the time he was a delegate to the 1984 National Democratic Party Convention. The author directs us to the most important questions in the presidential nominating process; his knowledge and understanding of black politics set the nominating process and the 1984 Jackson campaign into a broader framework, and his sensitivity to contemporary problems in the United States enables him to convey the feelings and reactions of one man to a political event that was important in his own life.

The book's main contribution is the recognition that Jesse Jackson in 1984 was a serious candidate for the presidency. Here the book foreshadows the 1988 presidential campaign in its presentation of Jackson's charismatic speaking ability, his uncanny knowledge of the most important issues facing the country, and his abiding respect within the black community. What he accomplished in 1988 he began in 1984.

A secondary contribution is its juxtaposition of social science theory with daily life experiences. As a political scientist, of course, Barker realizes which events at a national party convention are most important for the party, which for the candidate, and which for other black elected officials. But these events are not necessarily the most memorable or notable ones for an individual delegate. For example, Barker gave his floor credentials to an alternate delegate the night Walter Mondale and Geraldine Ferarro delivered their acceptance speeches. For him the session was anticlimactic even though the acceptance speech of a woman running for the vice presidency was historically significant and the acceptance speech of the party's nominee for the presidency was politically significant. In contrast, Barker describes his feeling of importance and influence in making the official Jackson presentation of the minority planks to the minuscule Guam delegation. Although that presentation was relatively unimportant in the overall scheme of the convention, for him it meant the opportunity to influence other people. Maybe the majority in that delegation would break rank and support his policy alternatives; he would watch the roll call vote. "That is the concrete index" of his influence.

Our Time Has Come is also an essay on the significance of a Jackson campaign for black politics. A black presidential candidate would "strengthen the role and influence of blacks and minorities," provide a "focal point around which blacks and minorities could rally," and "spur increased political interest and activity at local and state levels" (p. 17). Clearly, then, the author sees the candidacy of Jesse Jackson as more than one person's run for the presidency. Jackson is both a significant candidate and a symbol of a new era of minority leadership. Understanding these two roles helps us place the analysis of Jackson's 1984 campaign and of the 1984 Democratic convention within a context of a black political agenda that will be relevant for presidential elections into the next millennium.

When I read *Our Time Has Come,* I realized how unfortunate it is that more political scientists do not take advantage of participant observation. This methodology lends itself particularly well to capturing the color, the excitement, and the adventure of political extravaganzas, such as a national party nominating convention, while allowing the same genesis of ideas and rigorous tests of hypotheses that are common to regression analysis, formal modeling, and other quantitative techniques. This book reminds us that social science is a blend of several methodologies that can provide answers to a focused set of puzzles and problems.

No book on Jackson's 1984 campaign could avoid discussing the relations between the candidate and the Jewish community, and *Our Time Has Come* takes on the challenge. Jesse Jackson's low point in his public image was surely created by reference to New York City as "Hymietown" and to Jews as "Hymies." Barker correctly points out that these words are absent from the standard lexicon of anti-Semitic verbiage and that Jackson's comments were not intended for public consumption, but his analysis of the event adds little to our understanding of Jackson. In the first place, the absence of these words from the standard anti-Semitic lexicon is irrelevant to the point conveyed by the statement. Blacks in the United States would and should be offended by a reference to Washington, DC as "Samboville" (my impromptu coinage). Prior knowledge of a phrase is not necessary to deciphering its meaning. In the second place, the privacy of the intended audience adds to—rather than detracts from—the concern. Public statements by a candidate may or may not reflect true attitudes, since politicians often must moderate their own views to be elected. But private remarks reflect people's beliefs and character, and what people think is "funny" tells us about their character, personality, and concept of self (as Freud made us aware). The lingering question here is whether the Jewish community should ever accept Jackson's apology. I personally think it should, but I would like more help in knowing why.

Louis Farrakhan's support for Jesse Jackson also created tension between Jews and blacks in 1984, and here *Our Time Has Come* is illuminating but incomplete. This leader of the Black Muslims described Hitler as a "very great man" and called Judaism a "gutter religion" and Israel an "outlaw" nation. One has trouble defending him. Yet Barker is bold when he writes, "I appreciated why Jackson found it difficult to repudiate Farrakhan summarily. Indeed, one might suggest that Farrakhan represents or reflects an important strain of political thought in the black community" (p. 83). The paragraphs that follow describe Farrakhan's language accurately, calling it repulsive, and then offering an astute observation about white America: "Farrakhan's comments could become a convenient excuse, a means of criticizing Jackson without jeopardizing one's liberal or racial credentials" (p. 83).

In other words, to Barker the relationship between Jackson and Farrakhan in 1984 was complex. But Barker ignores a fact that adds to the complexity. Before Jesse Jackson was eligible for secret service protection, the Nation of Islam provided bodyguards for the candidate. Recalling Jackson's personal experience with violence, attacks, and assassination, we gain some sympathy for the unwritten pact between two recognized leaders in the black community. One praiseworthy aspect of Jackson, then, may be his unswerving loyalty—even in the face of controversy—to a person who helped at a time of potential danger. In 1988 George Bush, for example, continually receives praise for this aspect of his character.

In another issue—Jackson's mission to Syria—Barker misses an opportunity to remind readers about unfair treatment given Jackson. Widely criticized by Jews for directly negotiating with an Arab nation that often supports terrorism in the Mideast and also criticized by the news media for using the hostage issue to his own political advantage, Jackson deserves to have history revised. He was, after all, successful in obtaining the release of Lt. Robert Goodman and, what is more, he achieved the release without paying a ransom of either cash or arms. If negotiations with belligerent nations leads to peace or to the freeing of prisoners, all should applaud.

Barker's discussion of the 1984 Jackson campaign, in general, is a sympathetic discourse on the candidate. In effect, the book should have been titled *On the Right Side of History,* an often-quoted phrase from Marion Barry's speech nominating Jackson at the 1984 Democratic convention. At times, the author goes beyond his stated purpose of chronicling the events of 1984 to insist that Jackson's campaign be interpreted favorably. This should be required reading for those who did not understand Jesse Jackson in 1984 and have not figured out that this man was an important figure in the last two, and probably will be in the next several, presidential elections.

C. ANTHONY BROH

Princeton University

A Court Divided: The Fifth Circuit Court of Appeals and the Politics of Judicial Reform. By Deborah Barrow and Thomas Walker. New Haven: Yale University Press, 1988. 263p. $29.95.

"Judicial reform," the authors of this study remind us, "is not a sport for the short-winded." The saga of the realignment of the Fifth Circuit Court of Appeals testifies to the truth of this observation. In an incisive and scrupulously documented study of the politics of circuit reform, Barrow and Walker trace the eighteen-year series of events that culminated with the division of the Fifth Circuit and the creation of a new Eleventh Circuit in 1981. The authors argue that circuit reform was never a simple, administrative matter but rather was an overtly political solution shaped by the divisive political environment of the 1960s.

Discussion of Fifth Circuit realignment began in 1963, with the debate in the judicial conference over how to manage the southern circuit's increased caseload. Controversy within the judicial community over the feasibility of a court larger than nine members led to consideration of circuit division as a remedy for caseload problems. But division quickly became a point of political contention, due to the Fifth Circuit's record of activism on desegregation issues. Division would likely have a detrimental effect on civil rights, a point that escaped the attention of neither division opponents nor division proponents. Questions of whether and where to divide the Fifth Circuit became inseparable from political considerations.

A particular strength of this study is its detailed analysis of the judicial and legislative power relations that developed over the issue of circuit reform. The authors emphasize that the politics of the early period of debate established the tone of future confrontations over Fifth Circuit realignment. The study documents in some detail the maneuverings of conservative Senate judiciary chairman Eastland in his quest to split the circuit at the Mississippi River and thus to divide the court's staunchest civil rights advocates. Eastland proved to be a formidable adversary for division opponents, among whom were the liberal Fifth Circuit judge Wisdom. But by transforming a regional, administrative matter into an issue of national political concern, Wisdom and other division opponents forced Eastland into a lengthy political battle.

The study devotes much attention to the consequences of this political strategy of delay. Debate continued to rage throughout the 1970s between opponents of "circuit proliferation," civil rights advocates, and prorealignment forces; and as the arena of political struggle over the division issue continued to broaden, the court suffered the fallout of unresolved caseload and collegiality problems. The authors emphasize that the legislative impasse occurred in part because the Fifth Circuit judges were themselves divided over the realignment issue. By the late 1970s, however, events had conspired to facilitate a politics of accommodation on the circuit reform. Long a stumbling block to division, the civil rights argument against splitting the circuit had lost much of its applicability. In addition, 1977 saw the political retirement of two congressional adversaries in the realignment battle, Senator Eastland and liberal House judiciary committee member Jordan. Perhaps most significantly, by 1979 the Fifth Circuit judges were uniformly dissatisfied with the unwieldy procedures and diffusion of responsibility created by the addition of temporary judgeships and an increased court size. According to the authors, the court's unanimous reconciliation to the inevitability of division supplied a political factor critical for legislative reform action.

For the authors, circuit realignment was a specifically political solution, owing to the nature of the process of which it was a product. Moreover, realignment was a short-term solution to the caseload crisis, possessing only temporary positive results. The authors make clear that the political debate over circuit reform never centered on the *source* of the caseload problem. The need for jurisdictional reform as a long-term remedy for increased federal court workload was never seriously considered. The authors are somewhat critical of the various reform committees' collective failure to consider the consequences of the circuit division remedy on the circuit courts' "federalizing function." Their examination of the Fifth Circuit controversy illustrates dramatically that circuit division was a means to forestall—but not to solve—the caseload crisis in the federal courts.

The saga of the Fifth Circuit realignment ended officially with the passage of the Reorganization Act in 1980 and the establishment of a new Eleventh Circuit for the states of Alabama, Florida, and Georgia. The authors extrapolate from this saga a model for understanding the complex web of interinstitutional politics. They term the model of the politics of judicial reform one of "cooperative oversight" (p. 249). They argue that judicial reform necessitates mutual accommodation between the judiciary and Congress—accommodation that usually must await catastrophic problems. The authors conclude, somewhat reluctantly, that the cooperative oversight arrangement, combined with the lack of reelection capital that legislators gain from pursuing judicial reform, means that problems facing the courts will continue to be approached on a piecemeal basis.

For scholars concerned with both the jurisprudential and the political impact of the Fifth Circuit realignment, this volume offers valuable insights into the nature of the controversy and provides a useful model for studying the politics of court reform generally. As such, the study makes a welcome and needed contribution to the literature on courts and political culture. The model of interinstitutional politics and the wealth of political anecdotes extend the study's appeal to students of the legislative process. The study's rich use of archival sources and judicial interview data certainly merits high praise.

NANCY MAVEETY

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Critical Elections and Congressional Policy Making. By David W. Brady. Stanford: Stanford University Press, 1988. 212p. $27.50.

In the 1988 U.S. elections George Bush won 54% of the popular vote and 426 electoral votes, continuing the dominance in presidential elections of the Republican party, which has won five of the last six contests, four by landslides. Yet at the same time Bush saw his party's minority position in both Houses of the Congress weaken even further, reinforcing the Democratic party's control of political offices beneath the presidential level and the almost complete separation of the presidential and congressional vote. Divided government, with its attendant problems of political stalemate and policy irresolution, has become the norm in contemporary U.S. politics.

In an original and compelling piece of scholarship, David W. Brady reminds us of times that were different. In *Critical Elections and Congressional Policy Making,* Brady examines the process by which major policy changes were enacted during three critical periods in U.S. history: the Civil War, the 1890s and the New Deal. He is the first scholar to provide a comprehensive picture of the linkages among critical elections, political institutions, and public policy. His argument is straightforward: in response to a problem of intense national concern, voters replace the incumbent party with a new majority party, giving it control of the presidency and both Houses of Congress. With the normal impediments to legislative action weakened by heightened partisanship, favorable party ratios, and massive turnover on committees, the new majority party enacts policy changes that address the crisis and reinforce the new distribution of the popular vote.

Brady's empirical task is to demonstrate how electoral realignments create the conditions for majority party government in the House of Representatives. (The Senate is not a subject of investigation in this book, although the author states that its pattern of party control during critical periods is consistent with that of the House.) This requires that he show that in realigning periods (1) elections are decided more on national than local issues, (2) the constituent bases of the parties change, (3) committees experience sharp membership turnover, (4) party voting in Congress increases rapidly, and

(5) the issues associated with the realignment are dominated by the new and cohesive majority party. He is unusually resourceful in marshaling relevant evidence and exploring the implications of anomalous findings.

The author develops new methods for weighing the nationalization of political forces in House elections and interpreting the rate at which changes in the popular vote are translated into seats in the House. The differences among the critical periods in these two respects are as interesting as their similarities in fostering party government. Relatively minor regional shifts in the popular vote produced stable Republican majorities in the House and Senate during the two nineteenth-century realignments, while a much larger, across-the-board vote swing was necessary to produce a unified Democratic majority in the 1930s. A key difference between these two periods—and a crucial ingredient of realigning elections—is the shape of the constituent party distribution (i.e., the competitiveness of House districts). Brady demonstrates that the U-shaped distribution of House districts, labeled by David Mayhew for the contemporary period as "vanishing marginals," was present in the decade leading up to the 1932 election.

This book is an excellent example of the sophisticated social science research being done on U.S. political history. It helps us understand several key episodes in our nation's history, tests contemporary theoretical notions about elections, parties and policy making, and provides some much-needed perspective on the extraordinary period of divided government under which we now live.

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Televised Legislatures: Political Information, Technology, and Public Choice. By W. Mark Crain and Brian Goff. Dordrecht: Kluwer, 1988. 112p. $54.00.

Crain and Goff successfully accomplish two important tasks in this book. First, they thoroughly examine how televising legislative sessions influences both the competitiveness of political races and the behavior of legislatures. Secondly, they challenge the almost unanimous opinion by economists that politicians are constrained solely because of facing reelection. While the results from this second task could be made more compelling, Crain and Goff effectively challenge the received wisdom.

The empirical work starts with the interesting observation that televising legislatures makes it easier for constituents to learn of their representative's behavior and thus makes it more difficult to specifically target the information that a representative wants constituencies to obtain. Incumbents from districts with heterogeneous constituents then face more difficult reelection campaigns than those who represent homogenous districts, since helping out one set of constituents in heterogeneous districts may alienate other groups.

Crain and Goff trace many different implications from this single insight. Besides examining the effects of televising sessions on the probability of reelection, they demonstrate that the politicians who benefited the most from this change were the same ones who supported the enabling legislation. Those who benefited the least from televising sessions substituted other activities (e.g., passing legislation) and worked to reduce the length of legislative sessions, and thus of their exposure to television. Relying on state House and Senate and U.S. House and Senate data, the authors provide a wide array of qualitatively different tests.

The only possible weaknesses with the empirical discussions are the measures of district heterogeneity and the fact that in at least one case, alternative explanations are possible for their results. For the state-level data, heterogeneity is defined as the number of persons in a district. However, there seems to be relatively little reason to worry about variables that are correlated with population but independent of heterogeneity since the authors carefully examine whether these heterogeneous districts posed difficulties for incumbents before televised sessions were instituted. In the data for federal office, heterogeneity is measured by the difference in educational attainment. The authors find that districts with a greater proportion of less-educated people gave incumbents in 1980 more votes but that this had little effect on the number of votes received by winners in 1978. While, as the authors claim, this may be explained by the fact that the House introduced television between those two dates, it may also simply indicate that uneducated people vote for incumbents more systematically than they vote for winners.

The book also reexamines the mechanisms that exist to guarantee political performance. The typical notion views politicians as analogous to firms, where sunk investments in reputation guarantee quality and purchases are continuously repeated into the future. Consumers are presumed to pay more for products from firms with a greater reputation and if the firm cheats its customers it loses this premium. On the other hand, if a firm temporarily deceives customers into thinking that they are receiving a higher-quality product than they actually are, the firm will obtain some short-term gain for cheating. Firms will thus only defraud their customers when the present value of reputation is less than any short-run gains from deception.

More economists have simply substituted the words voters for customers, politicians for firms, and political reputation for brand name capital. However, Crain and Goff point out that this analogy does not work for one simple reason: politicians do not face reelection forever into the future and they have only limited ability to transfer this reputation to others when they do retire (see also Gertrude Fremling and John Lott, "Televising Legislatures," *Public Choice* 58 [1988]). Since cheating in these models only imposes a cost on those who fail to honor their promises to voters—by removing them from office—it will always pay for a politician in his or her last period to cheat. However, if the voters know that the politician is in his last term in political office, they will not vote for him in the preceeding election; and the politician will cheat in what had been the next-to-last term in office, and the whole process unravels. Observations that politicians occasionally announce that an upcoming election will be their last, the existence of tenure limits for certain offices, and that politicians face only finite lives, ensure that many voters will know when the last period will occur.

In summary, Crain and Goff have provided us with a well-integrated empirical study of the impact of televising legislatures. They have also convincingly questioned the notion that it is the threat of facing reelection that guarantees political performance. After all that, it is rather difficult to ask for more. The only possible requests would be an explanation of what, besides sunk investments, may guarantee political performance and a stronger linkage between the theory that facing reelection guarantees performance and the empirical evidence that supports it.

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The U.S. Press and Iran. By William A. Dorman and Mansour Farhang. Berkeley and Los Angeles: University of California Press, 1987. 272p. $30.00.

News As Hegemonic Reality. By Allan Rachlin. New York: Praeger, 1988. 168p. $37.95.

News Coverage of the Sandinista Revolution. By Joshua Muravchik. Washington: American Enterprise Institute for Public Policy Research, 1988. 128p. $19.75 cloth, $9.75 paper.

Content analysis as a research tool has been around for at least 50 years and has been more closely identified with communication research and theory than with the social science disciplines of political science and sociology. (The exception to this would be Adams et al 1986; Rothman and Lichter 1987; Paletz and Guthrie 1987; Nimmo and Combs 1985; and Feldman and Sigelman 1985.) However, the growing dominance of the mass media in political campaigns and in the public policy process in general has provided an environment in which social scientists are focusing more on media research and analysis. The political scientist who still views studies of the mass media as belonging only in communications journals might find it worthwhile to read these three books.

The books all analyze a variety of newspapers, but focus primarily on the national mainstream newspapers. All three deal specifically with descriptive accounts of newspaper coverage of events ranging from Iran and the 1978 revolution to a Soviet fighter plane shooting down a Korean Airlines flight en route from Anchorage, Alaska to Seoul, South Korea to the Sandinista revolution.

The research in Dorman and Farhang's *U.S. Press and Iran* and Rachlin's *News As Hege-*

monic Reality builds on previous work done by Robert E. Park (1939), Gaye Tuchman (1972, 1978), Erving Goffman (1974), Edward Jay Epstein (1975, 1981), Herbert J. Gans (1979), and Todd Gitlin (1980). A uniform theme of these two books addresses the general question, Assuming an informed citizenry is necessary for the U.S. democratic process, how well does the U.S. press serve democracy?

All three books share the common theme that the U.S. press is more influenced by ideological, cultural, and social inclinations than by journalistic judgments. However, the similarity between the three works basically ends with these comparisons. From that point on, the volumes' subjects diverge from the U.S. media and Iran (arguing that the media relied on government sources in covering Iranian events and issues from 1951 to 1978) to news reports about the Sandinista revolution (arguing that the U.S. press ignored official Washington's information that the Sandinistas were working to turn Nicaragua into a communist state) to the study of news as "hegemonic reality" (arguing that journalists' preexisting definitions of the world shape news coverage).

Dorman and Farhang use the *New York Times* as the foundation for their study. They also analyze all news reports published from 1953 to 1978 about Iran in the *Christian Science Monitor, Newsweek*, and *Time*. Other sources used were all news stories that appeared for the period of the study in magazines indexed in the *Reader's Guide to Periodical Literature*. Samples were also drawn from the *Washington Post, Wall Street Journal, Chicago Tribune*, and *Los Angeles Times*. Other various U.S. newspapers were screened in order to assess the news coverage provided by the two major wire services, United Press International and Associated Press.

A major conclusion of the book is that throughout the association of the United States with the shah, the media tended to defer to governmental authority by portraying political opposition to the regime in such a way as to suggest that the shah's critics were nothing more than benighted reactionaries (p. 3). According to the authors, the media consistently failed to look for hidden agendas in the shah's motives. Over the 25 year study, the authors could find only four uses of the word *dictator* in the mainstream news columns to describe the shah and rare use of the term in editorials: "How the shah emerged as merely 'autocratic' and Somoza as a 'dictator' is, in our view, at the heart of the matter" (p. 99). According to Dorman and Farhang, the news was framed so as to present a dichotomous version of U.S.-Iranian relationships; that is, the news was presented using themes of the "good" (foreign policy of official Washington) versus the "bad" (political and cultural aspirations of the Iranians).

As a result, the U.S. press projected a distorted view of Iran; this coverage was shaped by the foreign policy establishment using a cold war-oriented slant in defining events and issues concerning Iran. Consequently, according to the authors, the U.S. public was exposed to many damaging lessons that may take "years to unlearn" (p. 13).

Like *The U.S. Press and Iran, News As Hegemonic Reality* argues that U.S. news reports are consistently characterized by recurring themes and use or avoidance of "buzz words" that frame political reality for the news consumer. Accepting as a given that journalism and the press cannot "stand apart from the world on which it reports because the media are of that world," the author examines the role of political culture, or "hegemonic ideology" (p. 4), in defining journalistic accounts of the world.

Rachlin uses case studies of U.S. publications (*New York Times, Newsweek*, and *Time*) and journalistic reports outside the United States (Canada's *MacLeans's* and *Globe and Mail*) and the state-controlled Cuban *Granma* to analyze the Soviet shooting of KAL 007 on 31 August 1983, a strike of U.S. postal workers, and the Solidarity movement in Poland.

As with Dorman and Farhang, Rachlin finds recurring patriotic themes built around a point-counterpoint format. (U.S. news media deal with events by dividing situations into "dichotomous forces" [p. 81]). As a result, Rachlin finds the KAL 007 incident portrayed to suggest Soviet liability versus U.S. innocence ("murder and atrocity in the sky" [p. 37]); a strike of postal workers depicted as a "threat to the government and foundation of American society" and thus to "law and order" (pp. 96–97); and Solidarity projected in news accounts as representing antisocialist, anti-Soviet forces, the "good guys," with the socialist or communist government of Poland and their Soviet puppeteers as the "bad guys" (p. 119).

The Muravchik volume is less rigorous in its research scope. The study utilizes the *Washington Post, Wall Street Journal, Los Angeles Times,* and *Economist* and major TV network news. Although the reader is not informed of the method used in the news coverage sample, the notes indicate that journalistic accounts from the above sources were screened from 1978 to 1980. Muravchik concludes the TV networks "were less guilty than the print media of misportraying the Sandinistas" (p. 106). Within the print media, he concludes that the *Los Angeles Times* provided far more accurate coverage of the Sandinistas than the *New York Times.* The author is described in the preface as a "neoconservative writer on foreign policy." Indeed, the foreword is written by Pablo Antonio Cuadra, editor of *La Prensa,* a newspaper alleged by Sandinista leaders to have been secretly funded by the CIA (Michael Massing, "Nicaragua's Free-Fire Journalism," *Columbia Journalism Review,* July–August 1988).

As with any study using content analysis, the researcher is faced with numerous problems that tend to impair the validity of the work. One problem centers around the coder's reliance on frequencies of specific mentions in news accounts. As a result, quantitative analyses end up paying little attention to qualitative questions. All too often, content analysis ignores probing beneath the news story to determine questions such as, Why was this event portrayed in a negative or a positive manner? Who determined which officials were quoted and what were the sources for the information given?

Had the authors of these three books accepted Ole R. Holsti's (1969) argument that content analysis should be carried out using both quantitative and qualitative techniques, the works reviewed here might have made an even greater contribution to the literature. For example, had the researchers gone a step further and asked more pointed questions about how the news was determined in the news organization itself, the reader of these three volumes would have gained greater insight into the final news product analyzed here.

For example, Dorman and Farhang might have interviewed journalists, editors, publishers, and Washington officials to determine why the press "turned to the White House, the State Department, defense intellectuals, and members of the shah's regime for their understanding of the White Revolution, land reform, the religious opposition and the 1978 Revolution" (p. 214). Rachlin might have addressed the far-reaching implications of reporters' approaching a news story with preconceived dominant worldviews that included "Soviet liability and U.S. innocence" (p. 61). Muravchik might have examined more closely the dynamics of his central allegation that journalists must rely "on common sense and established procedures. . . . Yet these may not be adequate to cope with Communist governments or movements" (p. 107).

Nonetheless, these three books make their own contributions to a field of study that is basically untapped research into the mass media and coverage of U.S. foreign policy and international events. Though the press in this country receives much scrutiny, much of the work centers on one specific political campaign or one public policy decision. Further intensive review into the mass media, using both quantitative and qualitative approaches, is especially needed in the foreign policy arena. These volumes, particularly the Dorman and Farhang study over time and the Rachlin research (which goes a long way in assessing the interplay between U.S. political culture and news reports), add insight into the newsmaking process. This research will be especially useful to scholars interested in the concept of completeness and accuracy in the news reports on foreign affairs, which shape U.S. foreign policy.

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Latinos and the Political System. Edited by F. Chris Garcia. Notre Dame: University of Notre Dame Press, 1988. 528p. $29.95.

Latino Empowerment: Progress, Problems, and Prospects. Edited by Roberto E. Villarreal, Norma G. Hernandez, and Howard D. Neighbor. Westport, CT: Greenwood, 1988. 176p. $32.95.

Latinos in the United States: The Sacred and the Political. By David T. Abalos. Notre Dame: University of Notre Dame Press, 1986. 204p. $21.95.

It is estimated that within the next generation Latinos will be the largest minority group in the United States and will comprise a quarter or more of the population in several of the largest U.S. states, such as California and Texas. But political scholars have given rather little attention to Latinos in the U.S. political system. For this practical reason and for other reasons to be suggested, these books are welcome efforts.

While the works differ markedly in their focus and approach, they make important contributions. In some respects, the three books can be viewed as conversing with one another in order to come to grips with the historical and contemporary Latino-American political situation in the United States. The works both evidence shortcomings in their own right and reflect the paucity of systematic literature in the field which, in turn, seems to result from scholars of Latino politics' inattention to much U.S. political science and to the latter's inadequacies.

While all three of the works use the term *Latino* in their titles, none explicitly joins the debate regarding a preferred name for the group(s) under consideration. Yet that has been a seriously debated question, one thought to have significant theoretical and empirical implications. The three major Latino groups—Mexican-Americans, Puerto Ricans, and Cuban-Americans—are addressed in the Garcia book. In contrast, the book edited by Villarreal, Hernandez, and Neighbor—its title notwithstanding—examines only Mexican-Americans (Chicanos): and the philosophically oriented Abalos study speaks of Latinos in general without particular reference to subgroup differences.

Garcia presents, and provides summary comments on, 27 articles organized around five major topics derived from a systems framework: the political setting (i.e., history and demography), participation, representation and decision making, policies and issues, and outcomes and reactions. While the quality of the articles is uneven, a number are highly informative and well written and are linked to interesting questions and to significant political concepts. Such information and description are useful and necessary and are likely to serve as baseline findings for future research. A shortcoming, however, is that the articles rather seldom move much beyond (mere) description and data presentation toward the development of broader theory.

In contrast to most of the contributions to the Garcia book, several of the 10 articles in *Latino Empowerment* have a critical and theoretical tone, while several others have an assertive, advocacy flavor. How compelling one finds the feasibility and wisdom of the political programs and strategies advocated is less important than the fact that specific and general problems (and related plans) requiring attention are identified; that is, a (political) agenda is clearly implied in these articles.

Several of the other articles in *Latino Empowerment* use frameworks directing us toward theoretical approaches that differ from predominant views of minority politics issues. However, the initial evidence presented in behalf of these frameworks, particularly the more directly theoretical (as distinct from "advocacy" essays), sometimes seems scant, "soft," potentially inconsistent with other evidence, and thus not altogether compelling. Nonetheless, the ideas may serve as useful guides to future theoretical and empirical work.

Together, then, these two edited volumes provide commentaries that given their different strengths and weaknesses, complement each other. A related complement is that the Garcia book is more "behavioral" while *Latino Empowerment* is more "postbehavioral."

These two books imply a "politics of culture." But they do not themselves seek to address the need for broader theoretical, philosophical perspectives on Latinos and U.S. society and politics. In this regard, the Abalos work seems to inform the Garcia and Villarreal, Hernandez, and Neighbor volumes, if only indirectly.

Abalos is ultimately concerned with achieving justice through the "way of life of transformation," that is, the "creation together with our neighbors and sacred sources of new and fundamentally better relationships." This stands in stark contrast to the ways of life of "emanation" and of "incoherence." Emanation implies a way of life grounded in tradition(s) that, ultimately, are personally, socially, and politicaly stultifying; incoherence suggests that competition and the seeking of power dominate our social and political relationships. Abalos discusses the presence and implications of these archetypal ways of life for various aspects and institutions of Latino life and how the way of transformation can and should be adopted in guiding Latinos in choosing how to "create the present and future."

Abalos would imply, I think, that certain of the underlying assumptions of the Garcia and Villarreal articles as well as various efforts and proposed strategies presented therein are examples of the "ways of life" of "emanation" and "incoherence." Abalos is clearly sympathetic to various aspects of Latino culture; but he is careful not to romanticize about that culture because it embodies some (much) of the way of life of emanation. He might also be critical of Latino adoption of conventional power (pluralist) politics because it ultimately accepts the way of life of incoherence, which should be challenged. On the other hand, Abalos would probably applaud some of the policies and practices discussed in those works.

Abalos, a professor of religious studies and sociology, is to a considerable degree antibehavioral; he criticizes much social science literature for treating Latinos—and human beings generally—as "abstractions." At the same time, Abalos' view of the political goes well beyond the conventional confines. Abalos' approach and ideas will strike some as overly personal and biographical. He would respond to this by claiming that the self is the center of all significant transformation, "whether it be in the social, political, or religious realm." His views are also likely to be interpreted as utopian; given his specific topic, *quixotic* may be a more appropriate term. Perhaps a more legitimate observation would be that the work needs to more explicitly and extensively address itself to specifying and explaining the existing or essential institutions, processes and values that embody the ends and means he proposes.

Whatever the shortcomings of the Abalos work, it is bold, provocative, and worth reading. Further development of the ideas would probably be theoretically—and perhaps be practically—fruitful.

Thus, there is something of an interesting interchange perceptible between the three works. Although for reasons indicated earlier, some will question the relevance of Abalos' work for social *science* research. Continuing intellectual ferment along these lines is needed not only to enrich this subfield of inquiry but for what it can and should bring to the understanding of politics as a whole.

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A Life of Its Own: The Politics and Power of Water. By Robert Gottlieb. New York: Harcourt Brace Jovanovich, 1988. 332p. $20.95.

The pervasiveness of water problems and the perversity of water politics combine to create a policy arena that is at once fascinating and deeply troubling. How could something as elemental as water be allocated by a decision-making process that appears to defy logic? And, as Gottlieb points out, the arcane world of water policy tells us something about ourselves and our system of government: "To explore the way in which water has been used and misused becomes a way of ultimately exploring certain forms of politics and power in our society" (p. xviii). These "certain forms of politics and power," according to Gottlieb, smack of favoritism, conflict of interest, ideological rigidity, greed, elitism, and occasionally a grudging recognition of the need for reform. Gottlieb's basic assumption is that water policy can be improved when "new networks of community groups" (p. 240) and "new social movements" (p. 209) participate in the policy-making process. His distaste for the water politics of old contrasts with his support for "new agendas" (p. 203).

Much of the book is devoted to the politics of federal water development policy. There is also considerable discussion of California's water problems. The existing literature on these subjects is extensive, and Gottlieb adds little new information. He does, however, retell the story in a clear and eloquent style that can be appreciated by a wide audience.

The principal attraction of this book is the author's unique perspective on California water due to his role as an "insider-outsider" (p. xii). Gottlieb served on the board of directors of the Metropolitan Water District of Southern California—the "*MET*," which he characterizes as the personification of the good-old-boy club of water developers. His position on the board gave him his "insider" perspective. His "outsider" status resulted from his outspoken criticism of many MET policies and his alliance with environmental interests—the bête noire of the other board members. He was excortiated by his colleagues as a Judas in their midst.

His direct involvement in California water politics offers a fascinating look inside the California water machine. There is a price to be paid for this involvement, however; this is not a scientific study characterized by neutral objectivity. Gottlieb writes with the impassioned fervor of a reformer. His argument is convincing, but the reader must look elsewhere for the opposing viewpoint. In many respects this book is similar to several other recent books on water politics that offer a polemical treatment of water policy. The "water industry" and its allies are vilified in all of these studies, Gottlieb's included. He finds that the collusion of these interests—the Iron Triangle—operated in a dictatorial and self-aggrandizing manner until environmentalists and budgetary limits changed the basic nature of the game. Some of the events Gottlieb describes would push the limits of credibility were they not clearly part of a long-term pattern. The author is quite persuasive in convincing us that, yes, crazy things happen in the world of water politics.

Less convincing is his assertion that much of the irrationality of past years can be eliminated by large doses of environmentalism and civic consciousness. He assumes that the age of water pork barrel is about over, but he may be confusing concept with substance. To Gottlieb, pork barrel projects are the "structural solutions" (such as dams and canals) that environmentalists despise. If we are at the end of the dam-building era (i.e., the switch from development to management), we have by definition eliminated pork barrel. Gottlieb does not address the possibility that pork barrel can come in many forms, including expenditures favored by the "networks of community groups" he admires. For example, if it is pork barrel to build a dam, is it pork barrel to dismantle a dam? What about a project that aids, rather than destroys, fish and wildlife? And the biggest question—at least in terms of absolute expenditures—is the nature of government spending to improve water quality. In terms of pork barrel and iron triangles, what is the difference between a billion-dollar dam and a billion-dollar water treatment plant? Of course, the larger question concerns the relationship between public expenditures and democratic government based on geographic representation. Is some level of pork barrel inevitable in such a system, and are some policies more susceptible to pork barreling than others? If the answer is *yes*, pork never dies, it just changes form.

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Theoretical Issues in Policy Analysis. By M. E. Hawkesworth. Albany: State University of New York Press, 1988. 287p. $39.75 cloth, $12.95 paper.

Political theory and policy analysis have long appeared as divergent, even hostile, fields. Given that context, this is a refreshing book. Building upon the striking, recent emergence of a postpositivist genre in the policy literature, the author indeed holds out the prospect of a "theoretically sophisticated policy analysis" (p. 194).

The theoretical issue for Hawkesworth is the role that "theoretical presuppositions" play in orienting inquiry. Roughly, the first half of the book (chaps. 1–4), is devoted chiefly to a critique of presuppositions supporting empiricist principles, especially the dogmatic dichotomy between fact and value; the second half (chaps. 5–8) consists of exercises to identify and assess theoretical presuppositions in four cases. The author concludes (chap. 9) by articulating a theme that animates the book as a whole—that "the vision of policy analysis as science in service to democracy" (p. 184) is based upon unsupportable positivist assumptions and encourages a technocratic, depoliticized public life. A postpositivist orientation, in contrast, emphasizes a reflective dimension of inquiry, thereby focusing attention on deliberation. By shifting from "the rhetoric of science" to "the rhetoric of practical reason" (p. 190), this reflective mode of policy analysis would encourage "a more participatory conception of democracy" (p. 193).

Conventional policy analysis, Hawkesworth contends, takes too much for granted, including itself. The upshot of her discussion is to deny the possibility of directly apprehending an objective world and codifying it in strictly factual statements. Theoretical presuppositions persistently guide attention, perception, and thought; evaluation, moreover, is necessarily constitutive of these presuppositions. Accordingly, rational inquiry must remain always alert to what is being presupposed. This conclusion will not surprise anyone familiar with postpositivism, but Hawkesworth is right to emphasize implications for policy analysis. The case studies provide interesting illustrations and raise significant theoretical points. Although likely to be appreciated by many readers, these chapters unfortunately also divert the focus of the book from a sustained, direct treatment of theoretical issues.

Theoretical presuppositions, Hawkesworth maintains, are intrinsic to human cognition and do not primarily operate at a conscious level but, as "internalized commitments," structure thought in a way often difficult to recognize. Even consciously self-critical efforts frequently result in "caricatures that can be dismissed easily" because they fail to grasp fully and confront directly the real challenges (p. 84). The apparently pervasive character of presuppositions suggests the question, however, of what distinguishes a *theoretical* presupposition from any other kind. Indeed, the question becomes perplexing since the view Hawkesworth endorses takes theory itself to be "a constellations of presuppositions that provides a context in which perception and cognition occur" (p. 82). From within such a constellation, in any case, inquiry proceeds in a piecemeal fashion, drawing into question only particular elements at a time.

Rejecting both correspondence and consensus theories, Hawkesworth supports a coherence approach to truth, emphasizing multiple and essentially contestable criteria. Yet the logic of her position also suggests that coherence itself presupposes the fundamental consensus of an interpretive community (p. 52). Inquiry always remains entangled in traditional ideas. This is no fatal flaw, though, because theoretical constellations are "neither irrational nor arbitrary." Rather, they carry forward "collective wisdom" developed in the "thoroughly rational judgments of generations of people who have struggled to capture the truth about reality" (p. 85). Perhaps sensing a problem of idealism, however, Hawkesworth also strikes a note of realism. Theory is constrained by "stimuli in the external (or internal) world" (p. 53). While thus touching on an old dilemma, Hawkesworth does not clearly resolve it. Indeed, have elements of a correspondence approach not been ushered in through the back door?

Given Hawkesworth's reliance on devotion to truth in traditions of inquiry, a paradox emerges in her treatment of policy analysis itself; for she suggests that policy analysis "clings to a flawed epistemology" cynically, "not because of a sincere commitment" (p. 190). Hawkesworth also follows a strategy of homogenization that fails to take seriously crucial contributions to the policy literature. Since all present approaches betray commitments to similar untenable principles, all can be dismissed as we move on to fashion a postpositivist position. Such caricature may be useful in broadly assessing historical and political features of policy analysis, but not in methodological critique. Whatever overall appraisal one might make of figures such as Harold Lasswell, Charles Lindblom, and Herbert Simon, their work grapples insightfully with theoretical issues in policy analysis. Hawkesworth's book thus curiously lacks attention to discussions of theoretical issues, already present in the policy literature, which would be relevant to new approaches emphasizing democratic participation in an uncertain world.

With regard to the originator and chief proponent of the idea of "the policy sciences of democracy," Hawkesworth presents Lasswell, in the typical fashion, as a one-sided, cardboard figure. Considered in its complexity, however, Lasswell's thought reveals a uniquely sophisticated version of what Hawkesworth wishes to challenge; for there is not an empiricist principle mentioned by Hawkesworth that Lasswell would not either reject or dramatically reformulate. Central to Lasswell's idea of the policy sciences, moreover, is the proposal for a both individual and collective project of contextual orientation that would focus critical attention on what no one can avoid: the implicit understanding of self in context. Psychoanalytically informed and influenced by Lukác's early interpretation of the Marxian dialectic, Lasswell's concept of contextual orientation would indeed aid in advancing further Hawkesworth's valuable and insightful effort to expose the presuppositions of policy analysis.

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The Reagan Legacy: Promise and Performance. Edited by Charles O. Jones. Chatham, NJ: Chatham House, 1988. 314p. $25.00 cloth, $14.95 paper.

"It is not too soon," writes Charles O. Jones in his introduction to this edited volume of essays, "to begin an analysis, understanding that perspectives on [Reagan] and his performance will change over time." Change they will, but this stocktaking offers insight and information of enduring value.

The book divides the Reagan landscape into four parts: political institutions (featuring chapters on White House organization, the Congress, courts, and bureaucracy), political processes (one chapter dealing with Reagan's impact on Republican party fortunes, and one probing his standing with the public), public policy (notably budgetary policy and foreign policy), and a concluding "perspectives" section with one essay presenting "The View from Europe" and one assessing President Reagan's political strategy.

Edited books defy coherent summary, but a few themes illustrate the substantial (though not total) consensus that emerges from these 10 essays. One such theme is that Ronald Reagan profoundly altered the U.S. political landscape. By winning quick legislative approval for fundamental policy changes (Charles O. Jones), making an unprecedented number of judicial appointments (David M. Obrien), and maneuvering adroitly to effect the largest peacetime defense buildup in history (Paul E. Peterson and Mark Rom), Reagan became the only postwar president able to shape both domestic and foreign policy in the direction he preferred, while gaining reelection and keeping his party united (Aaron Wildavsky).

Another theme is that Reagan significantly refashioned the operational presidency—using the overhead agencies more forcefully than his predecessors (Bert A. Rockman) and streamlining government operations (Peter M. Benda and Charles H. Levine)—and distanced the presidency from many of the debilitating problems previously directed to the oval office (Aaron Wildavsky).

A third theme concerns the often-noted tendency to underestimate Reagan's political and leadership prowess. In his meaty and thoughtful essay on Reagan's public support fortunes James W. Ceaser offers a spirited and favorable assessment of the president's leaderly capacities. This view is shared by Aaron Wildavsky, who regards Reagan as an astute and underappreciated political strategist. More grudgingly respectful are Anthony King and David Sanders, who, noting how Europeans had sniffed with disdain at this president's Hollywood origins, and were finally obliged to acknowledge his abilities. Editor Jones enters an implicit caution by observing that expecting too little of Reagan sets one up to be overly impressed even with ordinary accomplishments.

Last, these generally favorable reviews are spiced with numerous adverse judgments and criticisms. Reagan fell short of bringing about his dreamed-for political realignment (Paul Allen Beck), he bears significant responsibility for budget and trade deficits of historic proportions (Peterson and Rom), he contributed to a serious erosion of the federal workforce (Benda and Levine), and his foreign policy approach (although it "worked for him") was not on balance helpful to the United States (I. M. Destler).

Does this conclusion of writings have shortcomings of its own? There might have been a better balance of topical coverage than four chapters on institutions and only two on policy. Certain Reagan accomplishments—such as his restoration of public confidence in the institution of the presidency—get only passing note. And there is no treatment of what Sidney Blumenthal, writing in another volume of essays on the Reagan legacy he and Thomas Byrne Edsall edited, calls the "neokitsch aesthetic." This was Reagan's creation of a unique, culturally and politically influential style—featuring optimism, ostentation, and patriotism—that set the tone for his era (*The Reagan Legacy*, [1988], p. 251).

Still, this is a useful group of strong essays by knowledgeable specialists who offer fine-grained summaries of the Reagan record in several key areas. The ultimate significance of the Reagan administration is not probed at length here. It could not have been, for the final verdict of Reagan's place in history awaited answers not available in 1988: How did the massive budget deficit, the foreign borrowing binge, and the trade imbalance eventually mark the economy? Did the United States find a way to finance a continuation of its role as leader of the free world? What became of Soviet-U.S. relations as a consequence of Reagan-Gorbachev summitry? What were the eventual impacts of the Reagan judicial appointments on civil liberties, social justice, and the place of government in U.S. life? Such are the questions future historians will use to rate Reagan. In their efforts to understand the workings of the presidency they will judge, those historians may find it helpful to return to the Jones volume.

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Liberty under Siege: American Politics, 1976–1988. By Walter Karp. New York: Henry Holt & Company, 1988. 255p. $19.95.

The Nationalization of American Politics. By William M. Lunch. Berkeley: University of California Press, 1987. 408p. $25.00.

These two books deal with more or less the same period in U.S. politics. There is some overlap (though not much) in the political orientations of the two authors. Otherwise, the books have almost nothing in common. Both may be useful, but for quite different purposes and to rather different sets of readers.

Liberty under Siege is a polemic. It has many of the virtues and most of the vices common to that genre. It makes no claim to dispassionate analysis, and it will add little to professional scholars of politics' awareness or understanding of the events with which it deals. For those who share Karp's perspective, as in considerable part I do, his book will serve to quicken the pulse, stir the gastric juices, and renew one's zest for the confrontation with the forces of iniquity. For nonbelievers, it may serve similarly to rouse their ire, stimulate their zeal, and prepare them emotionally for further political combat. There are no doubt those who have not yet made up their minds about the issues with which the book deals, but it will be irrelevant to them because they are most unlikely to read it.

Karp's thesis is that the achievements of the political Right in the 1980s owe less to Ronald Reagan and his acolytes than to mainstream leaders of the Democratic party, particularly in Congress, who constitute part of an oligarchy just as determined as the most ardent Reagan ideologues to keep most of the U.S. population in a state of political ignorance and impotence. The real villains are named O'Neill, Jackson, Byrd, Nunn, and Moynihan, aided and abetted by academics and interest group potentates such as Seymour Martin Lipset and Lane Kirkland. These are the people who doomed the Carter administration. (Here Karp is at his weakest, for he tends to ignore Carter's own essential—albeit decent—conservatism and his inability even to understand, much less control, the economic forces with which he was dealing.) These are also the people who ensured that the political battles of the 1980s would not be about the extent of the conservative domination but about which individuals and institutions would provide leadership for that domination.

Karp exaggerates, sometimes grossly. He lumps with his "oligarchy" virtually anyone willing to deal with the establishment and therefore virtually everyone engaging in politics. His rhetoric is so shrill that it may offend even those initially sympathetic. But for all that he has a point and one that even those who disagree may need to take into account.

Lunch's purpose is to explain the dramatic transformation in the nature of U.S. politics that has taken place over the last three decades. With regard to the specifics, he offers little that is new. Yet when these specifics are added up, a quite-valuable description of the current state of U.S. politics and an intriguing explanation of how it got to be that way emerges. For better or for worse, what the Madisonians have always feared and what Schattschneider and Lowi urged has largely come to pass. (Whether this change can or will produce the *results* Schattschneider or Lowi desired is more problematical.)

A nationalized politics more nearly ideological in character is what we now have. What produced this change? There is nothing particularly surprising in Lunch's answer: television, the growing affluence and political sophistication of much of the citizenry, and the decline of political parties, which had been historically strongest at the local and state levels. But while a great deal of scholarly attention has been devoted to each of these phenomena for at least a generation now, no other book has pulled together the disparate literature of these fields as well as Lunch's and then related these processes to their political consequences. Under the old scheme, all politics was "fundamentally limited, local, personal." But politics now is "essentially unlimited, national . . . impersonal, and informed by theory, if not ideology" (p. 277).

Lunch's book is not without flaws. Some of the observations are dated, suggesting that parts of the book were written long before the publication date and not brought up to date later. Not surprisingly, he is not equally conversant with all of the topics, necessitating a rather uncritical reliance on the existing literature in some areas. And Lunch is a scholar, not a journalist, so that his writing, while lucid and straightforward, lacks the fire of Karp's arguments. (For the scholarly reader, that may be to the good.)

On the whole, however, Lunch's book is first-rate scholarship and a very important contribution. For those of us educated in the 1950s, writing and lecturing now about a subject that seems to bear little resemblance to what we once observed, it probably does more than any other one book to tell us how and why a revolution in the nature of U.S. politics slipped up on us.

MURRAY CLARK HAVENS

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Judges and Legislators—Toward Institutional Comity. Edited by Robert A. Katzmann. Washington: Brookings Institution, 1988. 200p. $28.95 cloth, $10.95 paper.

In November 1986, prominent members of the judiciary and Congress, staffers, attorneys, and scholars met at the Brooking's Institution to begin a dialogue over relations between the branches of government. Acting on the premise that judges and legislators "lack appreciation of each others' processes and problems" (p. 1), the participants explored three aspects of interbranch relations (p. 4), with the hope of moving toward greater institutional comity: (1) "the prudential and constitutional reasons for the absence of communication between the branches," (2) "the institutional arrangements through which each branch presents its views and assesses the problems of the other," (3) "the kinds of practical steps that might be taken to improve judicial-legislative relations." Papers presented on these topics and ensuing discussions form the heart of this intriguing volume, *Judges and Legislators: Toward Institutional Comity.*

That serious problems of communication exist between the branches of government is a fact to which all the contributors submit. Moreover, they agree on the basic causes of the gap. On one hand, constitutional theory and practice dictate that the judiciary and Congress "interact" on a range of important matters, including "the entire spectrum of oversight, appropriations and the budget, investigation, program legislation, rules of procedure, the confirmation process, pay and perquisites, and impeachment, judicial discipline, and ethics" (p. 66). But on the other, the Constitution and its underlying features, such as separation of powers, are less than clear on the appropriate ways in which they might "communicate." As Judge Frank M. Coffin explains, "The Judiciary and Congress not only do not communicate on their most basic concerns; they do not know how they may properly do so. . . . The condition is that of a chronic, debilitating fever" (p. 7). This "fever" can have truly grave consequences, in the eyes of editor Robert A. Katzmann, because it can lead to poor public policy—laws that unwittingly increase the workload of judges and budget cuts that force the federal judiciary to "suspend" civil jury trials, to name a few (p. 8).

To begin a move toward greater institutional comity, the editor constructed this volume around the three core issues discussed at the Brookings colloquium. The first two chapters explore prudential and constitutional sources of the problem. Coffin begins the series with a clever piece entitled "*The Federalist* Number 86: On Relations between the Judiciary and Congress"—what he is "quite sure" Madison, Jay, and Hamilton would write on the subject if he "but had a better ability to hear them." In short, Coffin suggests inquiries into the possibility of "soften[ing] the edges where the judiciary and Congress rub" (p. 28) to allow for greater participation and communication between the branches, while maintaining the integrity of the separation of powers.

Coffin's suggestions seem particularly apt, given the conclusions reached by historian Maeva Marcus and Emily Field Van Tassel. Their examination of the relationship between judges and legislators between 1789 and 1800 questions conventional wisdom that the gap between Congress and the courts had its origins in the founding period. Using a range of primary sources, they report that traditional assumptions are only partially accurate. The Supreme Court as an institution did remain separate from Congress, but individual justices felt free to involve themselves in a range of "political matters" (p. 44). This institutional-individual distinction, they suggest, not only was a logical response—it maintained institutional integrity while permitting Congress to benefit from the considerable skills of those on the federal bench—but also may provide insight for today's legislators and judges.

The middle section of the volume is aimed at increasing legislators' and judges' knowledge of each other's institutional arrangements, capacity, processes, and problems. Representative Robert W. Kastenmeier of Wisconsin and Michael J. Remington, chief counsel of a subcommittee of the House Committee on the Judiciary, present "Legislators' Lexicon to the Federal Judiciary"—what every "conscientious" legislator should understand about the judicial branch. Political scientist Roger H. Davidson's selection is the converse, "What Judges Ought to Know about Congress." Both are excellent guides to their spheres of study. Indeed, although they were meant to provide, as Davidson writes, "critical tourists" surveys, they also could be used as introductory material to a range of courses on the governmental process.

The final contributions deal with the volume's overriding objective: to offer a concrete set of steps by which to bridge the gap between Congress and the courts. The first two describe how courts and legislatures interact in the states and in the British system. The final pieces summarize participant discussions of the core issues and present a structured agenda for further inquiries. In the final analysis, this agenda reflects the primary themes presented in the papers and articulated in ensuing discussions: a set of ground rules delineating appropriate lines of interinstitutional communication, ways in which Congress and the courts "can better understand" each other, and potential "mechanisms that can be devised" to achieve those objectives.

Katzmann has put together an excellent volume. All the contributions are highly readable and interesting. None stray too far from their stated purposes. Moreover, the book meets its objectives, examining problems existing between Congress and the courts within historical and institutional frameworks and offering fodder for further thought and "pragmatic" suggestions. I highly recommend it to all serious students of the governmental processes.

LEE EPSTEIN

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Welfare Theory and Social Policy: Reform or Revolution? By Phil Lee and Colin Raban. Newbury Park: Sage, 1988. 232p. $39.95 cloth, $16.50 paper.

Democracy and the Welfare State. Edited by Amy Gutmann. Princeton: Princeton University Press, 1988. 290p. $32.50 cloth, $12.95 paper.

Justifying the Welfare State: The New Right versus the Old Left. By David Harris. New York: Basil Blackwell, 1987. 181p. $39.95.

Though each follows a different path, the two books by David Harris and by Phil Lee and Colin Raban and the third edited by Amy Gutmann all arrive at quite similar conclusions. They argue, first, that an extensive and elaborate welfare state is necessary in a good society. Second, they agree that the present philosophical defenses for the welfare state contain both contradictions and oversights that work against the development of a broad popular consensus justifying the further extension and expansion of the welfare state.

David Harris's *Justifying the Welfare State* begins with an excellent review of the New Right critique of the welfare state, which he follows up with a thoughtful presentation of a variety of theoretical justifications from the concept of rationality, the concept of rights, and citizenship theory. Harris develops an impressive argument against societies that rely heavily on free markets. In place of free markets he offers a Rawlsian type of analysis that, he demonstrates, gains greater power when coupled with citizenship theory.

Citizenship theory states that people in a community have a right not to be exiled from the community by being excluded from access to the minimal resources that define and are necessary for full community membership. This theory provides the foundation for Harris's attempt to resolve a crucial weakness in the present defenses for expanding the welfare state. The weakness concerns a seeming contradiction of two goals. One goal is the objective of treating with respect all who need help in the community. This goal calls for giving assistance to those who, by no fault of their own, would otherwise be excluded from access to the minimal resources that define full community membership. A second, often contradictory goal is to encourage individual responsibility. Encouraging individual responsibility leads communities to attempt to distinguish (by way of selective means-testing or other nonuniversal devices) between those who are and are not personally responsible for their own exclusion. The contradiction is that these selective devices generally have the side effect of stigmatizing those who are assisted and thus undermining the respect with which those who justifiably need assistance have a right to be treated.

Citizenship theory, Harris argues, tells us that since people who are excluded by no fault of their own have a right to be treated with respect and dignity, selective devices are wrong because they result in stigma. Where possible, universal devices should be put in their place. What, then, should be done about those who in the absence of selective devices might take advantage so as to take welfare rather than work at decent-paying jobs? Referring to the writings of Titmuss and Townsend, Harris asserts that the large majority of people who need assistance have in fact come into that situation through no or little fault of their own. This leads Harris to conclude that nonselective devices are an appropriate solution and that "a sense of duty or community would be relied on to prevent or minimize abuse of the system" (p. 161).

Unfortunately, this solution may be at odds with the premise that Harris himself believes needs to underlie citizenship theory. He says, "The most satisfactory moral basis for citizenship theory is one in which a system of duties exists alongside or underlies the structure of citizenship rights" (p. 160). To grant rights while presuming without solid confirming evidence that the duties that underlie the structure of those rights are being met does not treat duties and rights as coequals. For all of Harris's careful and insightful analysis, he does not in the end provide a compelling answer to the contradiction found in welfare goals alluded to above. As a result, his answer is not likely to become a satisfactory foundation for defending or expanding the welfare state.

Democracy and the Welfare State, edited by Amy Gutmann, raises numerous additional issues about proposals to expand the welfare state. The articles by Donald Moon, Jon Elster, and Carol Pateman—all excellent analyses joining many others in the book—are especially relevant to the issue of citizenship theory and universality. Moon and Elster agree that citizenship theory constitutes an important theoretical argument favoring both the idea of assistance and the idea of universal, as opposed to selective, assistance. Universal assistance is a necessary, if not sufficient, condition for achieving an important goal of the welfare state, namely, to help build and maintain the self respect of individuals in a world in which one's own private actions cannot always bring this result about.

It would be nice to rely instead upon private action and responsibility by way of an effective full-employment policy, but the authors show that a full-employment strategy contains numerous limitations *even* if it successfully created full employment. Full employment does not overcome the problems accompanying the existence of many low-paying and dead-end jobs, prolonged illnesses (whether job-related or not), and hardships brought about by the temporary dislocations inherent in a dynamic private market economy. Thus, an extensive welfare state is needed. It should preferably be one based on universality in order to avoid the problems of stigma and indignity that Harris delineated.

In his analysis, however, Moon discovers that universality contains still another set of contradictions. In universal programs, Moon suggests, people above the minimum standard but still embraced by the universality of the program sometimes consume such a high proportion of the available resources that the programs not only often become far more expensive than selective programs but can, ironically, crowd out persons who are below the minimum standard, the very people who most need assistance.

Another problem both Moon and Elster raise is that certain suggested solutions, such as wage subsidies or public service jobs, often cannot deal effectively with matters such as low pay. In addition, they point out that these solutions do not necessarily address the objective of sustaining or building self respect. Elster argues that some workers may feel that wage subsidies to supplement low-paying private sector jobs would be demeaning. He also suggests that public service jobs not regarded highly enough to be generated in the private market generally do not bring esteem to those taking such jobs when they are created as public service jobs solely to substitute for unemployment. One wonders, however, whether those who are unemployed and poor and want to work don't feel even less self-respect without jobs than with work provided by the public sector. The results of recent workfare experiments suggest as much.

Carol Pateman's article, "The Patriarchal Welfare State," raises still another set of issues regarding the adequacy of citizenship theory as an answer to contemporary criticism of the welfare state. There is an important sense, Pateman claims, in which arguments about welfare based on citizenship theory implicitly or explicitly presume that males head families. Pateman shows that such arguments and the welfare policies resulting from them fail to address the stark reality that half of the poor families today are headed by females. In Pateman's view, both the meaning of citizenship and how this is differentially applied to males and females need to be reformulated so that the treatment of females in the theory embraces present realities.

Examining the welfare state from a socialist perspective, Phil Lee and Colin Raban reach conclusions that support Pateman's argument. Though believing in the necessity both of the welfare state and its expansion, the authors describe the welfare state in Great Britain since 1945 as one of disappointment and failure. Labour social policies have not had their intended effect. One important reason, they say, is because the concept of economic class as conventionally defined no longer effectively captures the real divisions between the classes. Sex, race, and ethnicity have also become central. Lee and Raban argue that a need exists for a new Marxism that can be applied specifically to concrete policy issues in a democratic setting so as to provide practical and realistic policy choices. According to the authors, this new Marxism, to be successful, will have to accept that sex, race, and ethnicity form a foundation enabling the strong to suppress the weak that is as important today as economic class itself.

JOHN E. SCHWARZ

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New Order of the Ages: Time, the Constitution, and the Making of Modern American Political Thought. By Michael Lienesch. Princeton: Princeton University Press, 1988. 235p. $20.00.

This book is a valuable contribution to the scholarship in early United States political thought and historiography. Lienesch enters the liberalism-versus-republicanism debate through a discussion of various conceptions of time and corollary political views during the period 1783–1800. On the Lockean-versus-classical debate that has dominated early United States studies for the past 30 years, Lienesch concludes that "the truth lies somewhere in between. . . . In the late eighteenth century, American political thought was in transition, moving from classical republicanism to modern liberalism. Yet the transition was inconclusive, neither clear nor complete, and the result was a hybrid mixture that combined republican and liberal themes in a creative but uneasy collaboration. . . . Traditional people at the advent of the modern age, they found themselves clinging to the ideals of the classical past, while at the same time admitting the practical advantages of modern times" (p. 7–8). He illuminates this "complicated conglomeration"—which, according to this book, afflicted no less than Washington, Madison, and Jefferson—through an examination of the various conceptions of time prevalent during the 1783–1800 period: creation, change, reform, development, experience, founding, posterity, and destiny. This approach at once clarifies elements of both liberalism and republicanism and opens new and original areas of inquiry. Lienesch's research on the latter eighteenth century relies principally on the popular speeches, sermons, and pamphlets of local clergy, lawyers, and politicians, along with the writings of national intellectual elites: Madison, Hamilton, and Jefferson. This treatment effectively employs Lance Banning's conceptions of political "persuasions," as opposed to "party" positions.

This is an impressive and important work offering original insights into the founding period and confirms much of my own work on Jefferson. However, one wishes that Lienesch had not accepted the classical republican paradigm so uncritically but had used his considerable talents as a theorist to challenge some of its problematical features, such as its vague conception of "virtue"; its casual lumping together of Aristotle, Cicero, and Machiavelli; and its paradoxical use of the ancient constitution. This lapse may be a result of the continuing pervasiveness of this paradigm, which forces an either/or stance with respect to liberalism and republicanism, though Lienesch's thesis actually (along with recent books by Pangle and Dworetz) begins seriously to challenge it. Also, despite a very sophisticated treatment of various conceptions of time in early United States thought, Lienesch freezes Thomas Jefferson in an early agrarian attitude (p. 89) that we know he modified later when seeking national independence and self-sufficiency through a balance of agriculture and manufactures. Someone like John Taylor of Caroline County, Virginia would have been a better exemplar of pure agrarianism.

Still, overall, this is a nicely written, balanced, and insightful book that contributes much to the ongoing discussion of the theoretical foundations of the U.S. republic. Lienesch displays an exceptional grasp of the major theorists as well as the minor U.S. writers and the secondary literature. Such thoroughness has been needed in this area of Early United States political thought and historiography, and it will undoubtedly place this book among the classics in the field by Bailyn, Pocock, Wood, Appleby, Kramnick, and McWilliams.

GARRETT WARD SHELDON

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The New American Politician: Ambition, Entrepreneurship, and the Changing Face of Political Life. By Burdett Loomis. New York: Basic Books, 1988. 288p. $19.95.

Burdett Loomis has produced an informative, well-written, stimulating, and frustrating book in *The New American Politician.* It is informative because Loomis describes members of the House first elected in the famous class of 1974, sometimes called the "Watergate babies." They were and are a distinctive group of politicians who have been important catalysts for, and participants in, institutional change in Congress, making them an excellent subject for study. Loomis focuses primarily on the House but extends his analysis to the Senate by following members of the class who were later elected to the upper house. His thesis is straightforward. Loomis argues persuasively that members of the class of seventy four are entrepreneurs in politics, meaning that they seek out opportunities both for political advantage and to influence public policy—but on their own, rather than working in larger social or political institutions.

It is well written. The text is clear and Loomis traces a number of developments chronologically, making them easy for students, as well as scholars, to follow. Moreover, Loomis includes many capsule profiles of the young politicians who are his focus, in a manner reminiscent of David Broder's *Changing of the Guard.* This contributes to the readability of *The New American Politician.* With these strengths, the book should be widely adopted for courses on Congress.

It is stimulating because the developments Loomis describes—particularly the decentralization of power on Capitol Hill with which the class of seventy four is identified—are centrally important to understanding the many recent changes in U.S. national politics. In addition, Loomis provides numerous quotations that should, at a minimum, enliven many a lecture on Congress. One of my favorites, which speaks volumes about the recent changes in congressional life, is from House majority leader Tom Foley (who is not a member but an ally of the class of seventy four: "Sam Rayburn could have walked down the streets of Spokane, Washington, without anyone noticing him. Tip O'Neill couldn't do that, and its very unlikely that any future Speaker will be anonymous to the country" (p. 77). As this quotation suggests, Loomis emphasizes the importance of the press and publicity, particularly television, for the members of the class of seventy four as they established their political identities. We are still struggling to understand the new role of the press as a mediating institution in our politics. The descriptions of press strategies and relations here will provide considerable raw material for those who attempt to construct theories in this area. Similarly, Loomis describes the self-conscious manner in which many members of the class of seventy four have attempted to extend their influence through the development of personal networks. His description is very interesting, but too often Loomis limits himself to description.

The New American Politician is frustrating because, while the description is excellent, there could be a great deal more analysis. Henry Luce is reputed to have once asked a simple question: "How is political science different from good journalism?" Luce's question is worth pondering while reading this book.

Loomis does reach beyond description at points, particularly in his discussion of the role of the press (chap. 4), but sophisticated readers will often be disappointed with the lack of discussion of the implications of change in Congress for other political institutions and participants. For example, Loomis emphasizes the self-starting, entrepreneurial character of most members of the class of seventy four; but beyond the congressional leadership (for whom life is now harder), he does not discuss the impact of this approach on the political parties. Similarly, the decentralization of power away from the full committees is described very well in chapter 2, but there is little mention of the implications of this decentralization for other institutional players, such as bureaucrats or presidents. Among the most influential works on Congress in recent years have been those by David Mayhew and Morris Fiorina, who have suggested that the preoccupation of members of Congress with reelection has deeply influenced national political life. It is unfortunate that Loomis mentions their work and ideas only in passing.

In short, there is much to praise in *The New American Politician*—a worthy addition to the literature on congressional life and change—but also much that is missing. Fortunately, the weaknesses of this book are sins of omission, which Loomis can remedy in his future work.

William M. Lunch

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Gays/Justice: A Study of Ethics, Society, and Law. By Richard D. Mohr. New York: Columbia University Press, 1988. 357p. $30.00.

The Rise of a Gay and Lesbian Movement. By Barry D. Adam. Boston: Twayne, 1987. 203p. $19.95 cloth, $8.95 paper.

De jure and de facto discrimination against gays and lesbians abounds. That discrimination, as Richard Mohr asserts, is exacerbated by the status of gays and lesbians as invisible minorities—"for if one is gay, to register a complaint would suddenly target one as a stigmatized person" (p. 27). In *Gays/Justice* Richard Mohr develops legal arguments against discrimination and calls for gay political action.

Dissatisfied with the legal foundation for the right to privacy as developed in *Griswold* versus *Connecticut*, Mohr argues for a firmer one. The Ninth Amendment requires judges to look for implicit, fundamental rights. Equal protection of the laws requires that these implicit, fundamental rights be protected as are the explicit provisions of the Bill of Rights. He concludes that "privacy rights permeate virtually the whole of the Bill of Rights" (p. 88).

Is sex included under the right to privacy? The legal and cultural obligation to have sex privately generates the right to sexual privacy. The state cannot simultaneously require people to be private and publicly expose them. Additionally, persons present at a sex act are either partners or intruders. It is inherently private as well as inherently special: "For ordinary persons—not mystics or adolescent poets—orgasmic sex is the only access they have to ecstasy" (p. 113).

For gays and lesbians the protection derived from the right to privacy, however, is not enough. The "widespread *social* prejudice against gays" as well as their invisible status causes "the virtual eclipse of political rights" (p. 168). Thus, Mohr argues for the extension of civil rights legislation to gays.

Pessimistically, Mohr points out that the last time the Supreme Court ruled in favor of a gay was 1962. Since "pragmatic" politics apparently don't work, Mohr argues for a politics that will bring dignity and pride: "The way to change the culture is to get it to face reality, the still-largely-hidden gay reality. The real dizzy queens and diesel dykes, not the ones of culture's collective fantasy, are okay, neither requiring apologetics nor warranting grimaces from gay leaders" (p. 333). Mohr argues against sacrificing dignity for expediency.

While Mohr's analysis is prescriptive, Barry Adam, in *The Rise of a Gay and Lesbian Movement*, traces the development of a gay movement from the 1890s in Germany to the present in the United States. In the nineteenth century a gay underground as well as a gay intelligentsia developed. Walt Whitman's homoerotic poems provide just one example.

The first gay movement, the Scientific-Humanitarian Committee, was established in Berlin in 1897. German gay life and politics grew and flourished from then until the 1930s. However, at that same time, gay and lesbian movements in other countries were largely closeted. Heterosexual hegemony characterized French life with the only public gay displays appearing in literature. While the trial and conviction of Oscar Wilde dampened the British movement, a cautious literature and discussion did develop. In the United States medicine repressively defined homosexuality.

In the 1930s repression returned in the more open cultures. The Soviet Union recriminalized homosexuality. Germany extended the definition of outlawed criminal acts to include "even homosexual fantasies" (p. 52). Germany's invasion and conquest of Europe led to the destruction of gay life and the imprisonment of gays in concentration camps.

After the war, European gays freed from the concentration camps found that homosexual acts were still illegal. U.S. gays were subject to witch hunts in the 1950s. But a surprising change came when the British Wolfenden Commission recommended that homosexual behavior between consenting adults be decriminalized. With that, a British homophile movement developed to support the Wolfenden recommendations.

The New Left movements provided the resources and ideology for the development of the gay liberation movement. After a series of police raids on bars, stonewalling symbolized the new era of gay politics, because of "the reaction of the drag queens, dykes, street people, and bar boys who confronted the police first with jeers and high camp and then with a hail of coins, paving stones and parking meters" (p. 75). New leaders emerged from the New Left movements, not from the homophile movement. Following quickly the U.S. initiatives, gay liberation movements developed in Canada, Europe, and Australia.

The efforts of Anita Bryant, the assassination of Harvey Milk, the rise of the television preachers, the extension of fundamentalist religion, and the power of the New Right signaled a renewed attack on gays. Nevertheless, Adam notes that "it is perhaps a sign of the maturity of gay and lesbian movements of the 1980s that activists have sufficiently learned the rules of the game to have begun to make significant headway through law reform in many countries" (p. 121). Although the movement has not been fully successful, he sees a "coming out everywhere" (p. 134). Adam's conclusion, however, is similar to Mohr's: "It is the rush to appear 'respectable,' to have continually to justify one's existence, and to cope with the 'scandals' invented by the opposition that . . . fetters the full creative potential of relationships that are at once pleasurable and entirely voluntary" (p. 166).

At the 1987 meetings of the American Political Science convention, a roundtable on AIDS did not schedule a gay representative as a speaker. While gay love is gradually changing from the "love that dare not speak its name," gay politics is still politics about which political scientists neither speak nor write. With the exception of Dennis Altman, we have to turn to historians such as John D'Emilio and John Boswell, journalists such as Randy Shilts and Jonathan Katz, the public health experts such as Ronald Bayer to understand gay politics and issues. Thus, sociologist Barry Adam's *The Rise of a Gay and Lesbian Movement* and philosopher Richard Mohr's *Gays/Justice* are welcomed additions to the literature.

DAVID C. COLBY

American Cultural Pluralism and Law. By Jill Norgren and Serena Nanda. New York: Praeger, 1988. 235p. $45.00 cloth, $16.95 paper.

In their 1976 concurrence to *Moore* versus *East Cleveland* (431 U.S. 494, 1976) Justices Marshall and Brennan describe the tragic "cultural myopia" of mainstream society (p. 211). Their opinion was and remains an eloquent judicial account of how institutions—in this case the East Cleveland legislature's passage of a discriminatory housing ordinance—reveal an unconscionable insensitivity to the cultural diversity of the black family. Jill Norgren and Serena Nanda's book, *American Cultural Pluralism and Law,* examines this historical tradition of systemic intolerance to disenfranchised groups and cultures. The focus of the authors' argument is how law interacts with cultural pluralism. It is a book born out of the collaboration of political science and anthropology's contribution to the study of law.

Norgren and Nanda have written a compelling book that traces the paradoxical history of both the law's protection of, and yet tragic betrayal of, minority rights. Their account poses important questions on pluralism and law. How do courts mediate competing claims of minority interests? How do courts reconcile minority demands with the needs of the more dominant culture? How do culturally powerless groups use the legal processes to vindicate and remedy their rights deprivations?

Norgren and Nanda take the reader through the major historical annals of the law's dealings with culturally diverse populations. They explore this chronology in sections on race, religion, gender, and community, using critical analysis and relevant case law to document this legacy. Judicial opinions, as the texts of legal history, are valuable as first-hand insights into the conscience and psyche of a particular era. The majority opinion, for example, in *Korematsu* v. *United States* (323 U.S. 214, 1944), in which the Supreme Court upheld the exclusion and internment of Japanese-Americans, is illustrative of the fear, the hideous prejudice, and the cultural xenophobia of the period following the bombing of Pearl Harbor. In the chapters on religion, the Court's plenary opinion in *Minersville School District* v. *Gobitis* (310 U.S. 586, 1940) tells of the pervasive sentiment of distrust toward the minority claims of the Jehovah's Witnesses at the onset of World War II.

Much of the analysis on race, religion, and gender should be quite familiar to readers of constitutional law. However, the section on Native American "oratory," with original speeches documenting pleas for white understanding of Indian diversity, is a novel addition to the traditional literature on race and ethnicity. A discussion of the law's recurring confrontation with conflictual demands from secular humanists and fundamentalists is a refreshing supplement to the material on religion. But while the first part of the book examines old territory, the later section, "Community," presents an innovative perspective on pluralism and law. In their discussion of how legal manipulations, such as zoning laws and language requirements, reinforce the values of the dominant society, Norgren and Nanda offer new ways of thinking about cultural oppression. These examples of insidious and subtle methods of exclusion are often more instructive than lessons of overt malevolence to racial and ethnic groups by direct means (segregation, internment). A final chapter on the use of alternative conflict resolution strategies by minority groups is informative and well-documented. As Norgren and Nanda suggest, "American pluralism expresses itself not only in the different issues brought to courts for resolution, but also in decisions not to use courts at all" (p. 229).

Apart from the substantive material, there are a few points the authors might have considered. One conspicuous omission in a work that explores how courts, in particular, accommodate the claims of minority groups is Justice Harlan Fiske Stone's now famous note 4 from *United States* v. *Carolene Products Company* (304 U.S. 144, 1938). Stone's note, calling for judicial protection of "discrete and insular minorities," is an excellent compendium of why minority rights must be safeguarded within the larger political arena. Secondly, in a work that stresses the cultural integrity of different populations, it seems unwise to use the word *subculture* to describe these groups. The reference to subculture, particularly in a teaching text, implies a set of values that are not equivalent to those of the dominant order. Finally, I would disagree with the authors' conclusions that courts, especially the Supreme Court, must necessarily "steer a middle course" and "calibrate" the claims of minorities with the interests of a larger population (p. 235). The Supreme Court should be free of such political constraints and pursue, as Justice Stone wrote, a judicial activism that seeks to secure the rights of those who are frequently unprotected by the majority process.

Norgren and Nanda have written an important book that isolates the theme of cultural pluralism in the context of law. It is well written and insightful and may help to dispel the "cultural myopia" and suppression of pluralism so accurately described by Marshall and Brennan in their concurrence with the *Moore* v. *East Cleveland* mandate.

JANIS JUDSON

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What Process Is Due? Courts and Science Policy Disputes. By David M. O'Brien. New York: Russell Sage Foundation, 1988. 272p. $22.50.

Critics of judicial policy making argue that judges are singularly unsuited to make broad societal decisions because of their lack of expertise and the idiosyncratic pattern of law cases in which judges respond to individual demands for fair treatment. Others caution against the dominance of technical experts and urge the continued use of lay judges to counterbalance inequities that arise when an unrestrained technocracy controls public policy. The balance shifts continually between a preference for substantively "correct" decisions reached by technical experts and democratically derived solutions with public participation facilitated through the courts' insistence on due process.

The tension between the need for informed, rational decisions and the desire for democratic participation in policy making exists in all policy areas, but is particularly evident in areas such as environmental regulation, public health, and product safety, where scientific evidence is crucial. David M. O'Brien takes this dilemma as his thesis and investigates the role that courts play in overseeing the balance between expert judgments and the democratic process. He argues that there is seldom certainty about the level of risk that exists, for example, in the proximity of nuclear plants to residential areas or in pesticide residues left on foods. Since experts rarely agree but policy must be made, courts are often relied on to make pragmatic decisions to resolve disputes over such issues.

O'Brien discusses the courts' role in three contexts: the private law of personal injury, the trial courts' enforcement of specific regulations, and the appellate courts' struggle to oversee agency regulations. In the common law of toxic torts, he documents the courts' tendency to impose strict and joint liability in the face of increased risks to the public from the market's failure to internalize third-party costs from technological advances in the last four decades. He illustrates the common law solution with the Johns Manville case, in which many courts found the company liable for exposing workers to asbestos but the corporation went bankrupt to avoid paying compensation.

O'Brien also explores the recent development of public laws (such as the Clean Water Act) designed to prevent damage by setting limits to behavior before it can create liability. He uses the Reserve Mining case to illustrate the problems facing trial courts in attempting to enforce such legislation and the difficulty of reaching a settlement when multiple interests are involved, each of which believes in the absolute rectitude of its own position. Finally, he explores the oversight courts are asked to exercise in agency decisions through administrative law by limiting expert discretion in agencies to set regulations. He demonstrates the arbitrariness of the Supreme Court in overturning the Labor Department's benzene standard while upholding its cotton dust standard. He also reviews the lengthy debate between Judge Levanthal's hard look at the substantive content of administrative rules and Judge Bazelon's desire to increase procedural requirements to force more pluralist participation in the establishment of rules.

O'Brien assumes a neutral position in this continuing dilemma for the U.S. political process. He acknowledges the fallacy of certainty in scientific data; but he also recognizes the ignorance of judges, which he believes is as important in social welfare issues such as abortion and school integration as it is on science policy issues. He raises the question, What balance should be struck between expert judgment and the pluralist process? but affords no answer to it. He illustrates his description of the problem with landmark cases, but these may be atypical and not demonstrative of any pattern overall. This begs the question whether there are systematic differences between the decisions of administrative agencies and those of courts on the same issues. Does the courts' concern about equity in individual cases enable a few to obtain disproportionate benefits and destroy the compromises reached by agencies trying to view the larger picture? Or, as many critics of pluralism argued in the 1960s, is it so likely that agencies will be captured by their clientele that it is essential that courts remain a counterbalance to this tendency? Which is the more democratic institution of government: agencies, which represent the policies of their elected executive, or courts, with their ability to respond to opposing groups' demands? What we need is some systematic empirical research into these kinds of questions. In the meantime, O'Brien has done us a service by summarizing the arguments on both sides.

LETTIE MCSPADDEN WENNER

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The Politics of Program Evaluation. Edited by Dennis J. Palumbo. Newbury Park: Sage, 1987. 309p. $35.00 cloth, $16.95 paper.

Perhaps the most significant change in evaluation theories and research has been the inclusion and systematic explication of the political aspects of evaluation. Indeed, in the 1970s the skepticism of political scientists over the usefulness of evaluation research in enhancing theoretical developments and empirical research was articulated in terms of lack of relevancy to the dominant paradigms. How could evaluations of workfare, compensatory education or conservation programs conceivably contribute to the study of politics?

This excellent volume, sponsored by the Policy Studies Organization, closes significant conceptual gaps between politics and evaluation. It illuminates major aspects and relations between politics and evaluation and their analytical, methodological and practical consequences on evaluation research.

In the comprehensive and insightful introductory chapter, the editor Dennis Palumbo critically discusses the various definitions of evaluation, the multiple and diverse uses of evaluations, and the conflicting imperatives of evaluators and bureaucrats. To administrators evaluations are instruments of power to be exploited in the numerous power struggles that take place in bureaucracies. The "political dilemma facing evaluators is to steer a course between recognizing the political reality of evaluation and retaining the symbolism of neutrality" (p. 20). Palumbo proposes several frameworks for incorporating politics into evaluation, the more general of which calls for relating the seven major types of evaluation research (the information cycle) to the stages of the policy cycle.

Whereas political scientists have been skeptical about the contributions that evaluation research could make to the study of politics, evaluators resisted the systematic inclusion of political considerations in their studies. As Carol Weiss, a pioneer in the field, points out in chapter 2, the subject of politics seemed to be taboo in professional meetings during the formative years of the field. Yet, evaluation takes place in a political context and politics influences evaluation activities in three major ways: the evaluated policies are outcomes of political decisions; evaluation findings compete for attention with other considerations that influence policy makers; and evaluation, by its very nature, makes explicit and implicit political statements about a variety of issues, including the worth of programs and the legitimacy of program goals and implementation strategies.

The four chapters in part 2 discuss the links between politics and evaluation from the policy cycle perspective. In chapter 3, Chelimsky proposes an interesting framework for linking the various types of evaluation with the information needs of managers and policy makers at the federal level. She differentiates between three general purposes that evaluation may serve—policy formulation, policy execution, and accountability—and the type of evaluations that may meet the user's needs in each category. Evaluations are to be designed around the user's specific information needs, and it is through users that they enter the political process.

Patton's utilization-focused evaluation approach (chap. 4) derives from the premise that use should be the major concern of evaluation.

This approach is explicitly political in its emphasis on enhancing utilization by intended users: "Identifying intended users and agreeing with them on the intended use requires upfront choices—inherently political choices—about whose interests will focus the evaluation" (p. 115). At the same time, the integrity of evaluation depends on high-quality data and rigorous methods. Browne and Wildavsky (chap. 5) critically assess the imperialistic claims of evaluation: evaluators have tried to play too many roles, acting as policy actors, program managers, and implementors. Although they gained a sense of power and responsibility, "a sense of differentiation, appropriateness, and hence professionalization" (p. 167) has been lost. In practice, implementation and evaluation activities overlap. However, it is important to maintain the conceptual distinction between the two "because they protect against the absorption of analysis into action to the detriment of both" (p. 169). Policy termination, DeLeon persuasively argues in chapter 6, is the most problematic of policy activities and it must be viewed as political rather than analytical decision making. Politics is the arena in which evaluators have to exercise their skills if their evaluations are intended to influence termination decisions.

Part 3 focuses on epistemological issues and research methods. Guba and Lincoln reject the mainstream positivist approaches and advocate "fourth-generation evaluation" grounded primarily on relativist ontology and naturalistic methodology. Scaff and Ingram raise the fundamental question of theoretical import: How do theories influence observations and what are the paradigmatic and political consequences of specific theoretical choices? In the case of rational choice theory, the basic assumptions are idealized and "market" solutions are inevitably preferred over other competing values such as "social justice." The negative consequences of relying exclusively on a single policy tool are discussed by Tolchin. She questions the validity of using cost-benefit analysis as the major rationale in regulatory policies because it omits the human factor, with unfortunate outcomes for the individuals and groups who depend on government protection. In chapter 10, Kelly discusses the new approaches to policy research and attempts to synthesize them with the argument that they all share the goal of promoting an "improved democracy" and specific programs.

This volume, like the series of which it is a part, is an important addition to the public policy literature. It also constitutes a significant contribution to the study of policy evaluation.

DAVID NACHMIAS

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Why Americans Don't Vote. By Frances Fox Piven and Richard A. Cloward. New York: Pantheon, 1988. 325p. $19.95.

When George Bush was elected president, only half of the adult population cast votes, virtually the lowest U.S. turnout in the twentieth century. The 1988 low culminated a trend of steadily falling participation since 1960 and, in historical perspective, a long-time decline since the nineteenth century.

There are many difficulties in explaining turnout trends. Even the data to analyze the problem are unclear. There is the historical anomaly that turnout has been falling even as barriers to participation have decreased with the decay of racist legislation, increased education, and easier registration. Different theories compete. Low U.S. turnout has been attributed to individual voter characteristics, such as limited information, disinterest, and alienation; to the weakening of political parties and other agents of voter mobilization; and to institutional barriers to electoral participation, especially the remaining registration and residence requirements.

Piven and Cloward offer a comprehensive explanation, grounded in their well-known work on poverty and social movements. They argue that the underlying cause of low voter turnout is that U.S. politics is primarily oriented toward the interests of economic elites and gives limited attention to the problems of the disadvantaged, particularly blacks and the industrial working class. Historically, they view the various causes of decreased turnout as mutually reinforcing. As formal barriers to voting (e.g., white primaries and registration requirements) developed, "the parties turned away from the issues and campaign stratagems needed to win lower-class support. . . . The resulting marginalization of working people not only from political influence but from the political culture created by the parties in turn reinforced their tendency to abstain" (pp. 18-19).

This argument is developed in a number of directions, not always well integrated with each other. One tack is a discussion of the competing theories, grouped as "institutional" and "social-psychological." Piven and Cloward contemptuously dismiss the latter explanations as "normal science" and "the witless use of multiple correlational analyses," asserting instead that "the political context *determines* whether these factors will have a significant effect on participation" (pp. 117-20).

The bulk of the book is a historical review of turnout trends, centering on "the changing shape of the American political universe" at the turn of the century. A comprehensive review of the literature relies especially on the work of Burnham, Kleppner, and Shefter. It is then brought up to date by insightful analyses, both descriptive and statistical, of the effects of the civil rights and antipoverty movements and the registration campaigns of 1984 by both Democrats and Republicans. They conclude that most liberalizations of voting laws have had little effect, that the national Democratic party is unable or unwilling to expand turnout, and that Republican party technology has reinforced the social class bias of the electorate.

The final—and most pointed—element of the book is reform. Arguing the common thesis that registration requirements are the chief barrier to higher turnout, the authors champion their own organizational creation, HumanServe. It has spearheaded efforts to enroll voters through public and private agencies serving poorer and minority citizens, such as unemployment, health, and welfare offices. Recounting the obstacles to this effort (and providing an excellent background legal memorandum by Cynthia Williams), they see their program as the most promising route to a more just politics.

Piven and Cloward present a well-researched and well-written case on behalf of the consensual goal of increased voter turnout. Nevertheless, their book has many disputable arguments. In using historical turnout figures, they essentially ignore the data problems created by vote fraud. In using contemporary figures, they give inadequate attention to findings that special registration efforts, such as that of HumanServe, exaggerate and duplicate their results and that many of those who are registered still do not vote. In examining the literature, they neglect the theoretical basis of the effect of education on turnout.

Their most serious problem is the assumption that class differences are the inevitable basis of political conflict. Given this assumption, they are usually confident (but not in their epilogue) that more voting by disadvantaged groups would inevitably lead to a higher vote for the Democrats or some New Left party and to more egalitarian social programs. The reality is less comforting. Political alignments are so often based on ethnic, religious, and cultural conflicts. These alignments were the basis of urban machine mobilization of voters in the nineteenth century (recognized but deplored by Piven and Cloward) and remain the basis for politics today in many nations with high voting participation (e.g., Ireland and Israel).

Within the contemporary United States, there is no assurance that more voting by lower-status voters would change political results. There are no significant differences in political attitudes between voters and nonvoters; indeed, nonvoters even seem slightly more conservative. It is well to recall that Ronald Reagan would have won reelection in 1984 even if the electorate were confined to voters with *less* than an annual income of 20 thousand dollars and that George Bush in 1988 was *more* favored by nonvoters than by voters. Of course, these opinions could change in a dynamic political situation; and the root principles of democracy still call for increased participation. But political analysis should be based at least as much on evidence as on hopes.

GERALD M. POMPER

Rutgers University

Common Decency: Domestic Policies after Reagan. By Alvin L. Schorr, with a contribution by James P. Comer. New Haven: Yale University Press, 1986. 216p. $20.00 cloth, $9.95 paper.

The Limits of Social Policy. By Nathan Glazer. Cambridge: Harvard University Press, 1988. 192p. $22.50.

Economic considerations dominate contemporary debates over the most appropriate size and shape of the U.S. welfare state, and they will continue to do so as long as enormous deficits limit social policy. Yet these two authors of very different persuasion rightly remind us that moral and political considerations have their place in these debates. The general well-being of citizens is, after all, the object of the welfare state; and the design of programs and policies no less than their particular aims must be judged with that end in mind.

For Alvin Schorr, these judgments turn on what we owe those outside our circle of family and friends. This is fundamentally a question about the principles of decency that govern a community of strangers, principles that ought to inform our efforts to promote the general welfare by political means. He names five policy commitments that characterize decent communities; they involve a dedication to fair income shares, mainstreaming (the opposite of means testing), full employment, selective decentralization of authority, and integration.

Schorr does not offer a philosophical explication of these commitments or their relation to decency itself. His is not a general case for the welfare state. Rather, it is an effort to show which programs and policies are implied by these commitments and to offer very specific suggestions for reforming social security, public assistance, housing provision, and health care delivery. (James P. Comer does the same for education.) All of this is done from a self-consciously liberal point of view. On the dust jacket, Robert Lekachman, with considerable prescience (given the publication date of book), describes Schorr's recommendations as "a realistic social agenda for the Democratic party's 1988 presidential campaign."

What makes Schorr's book suitable for politics is the apparent resonance of his concrete suggestions for reform with a national community of citizens who understand what decency requires. Of course, if such a community does not exist, it will be impossible to create and sustain a new U.S. welfare state by appeals to decency. Recognizing this, Schorr sometimes suggests that such a community may be called into existence by shaping our institutions in a way that cultivates decent values in the citizenry, which immediately raises questions about the relation between policy analysis and politics, since that is where the process of shaping institutions presumably begins.

Glazer's iconoclastic professional views are apposite here. Those who know his other work or have read earlier versions of these essays will not be surprised to discover that he doubts the existence of a citizenry animated by common values. By his telling the United States has no national community, at least in any strong sense; it is instead a nation of communities diverse in racial and ethnic backgrounds. Sustained by their traditions (what Lévi-Strauss called "superstitions") these communities hold values antithetical to the rationalizing efforts of those committed to radical egalitarianism of the sort represented by Schorr.

At one level, this augers against efforts to expand the welfare state in the United States. Ordinarily, citizens do not support this project. Only in extraordinary times does their resistance break down and then only partially and temporarily, producing an incomplete national welfare state open to local variations in policy and practice. But at another level this is a fact to be celebrated, albeit cautiously, according to Glazer. Families, neighborhoods, churches, and associations make up the "fine structure" of society. As such they are an important middle term in the relation between the individual and dominant institutions like the bureaucratic state (and the welfare state cannot be anything but bureaucratic, if it is truly national, according to Glazer). These associations offer some protection against "the arrogant impertinence of the administration"(an expression borrowed from Lévi-Strauss's *New Society* (1977) essay, "Reflections on Liberty," which in turn quotes it from Renan's *La réforme intellectualle en France.*)

Valuable as they are, human associations are also fragile. Their existence is threatened by modernization. They are additionally threatened by entitlement programs, which replace social bonds with formal, often legal, relations. Even worse, the programs most responsible for this inadvertent erosion of communities have failed to achieve the decent goals they sought (the War on Poverty did not end poverty, etc.). Glazer's criticism on this score is only partly rhetorical. His criticisms of compensatory education and employment training programs—and even public assistance—are quite specific and not too far removed from the objections raised by Schorr himself.

Glazer does not consider how much worse-off we would be had these "failed" programs not been enacted, although his views can perhaps be inferred from his unwillingness to dismantle the programs he finds wanting. Instead, he suggests that better results might be obtained by a greater reliance upon decentralized delivery systems or even private organizations more in tune with community values, anticipating George Bush's celebration of "a thousand points of light" over and against the searchlight of a national welfare state. Unfortunately, Glazer does not extend his intense scrutiny of existing welfare programs to include the possible deficiencies of his preferred alternative. Considerations of equity receive particularly short shrift from Glazer, as problems of nonselective or selective provision under these arrangements are elided in favor of endorsing the undeniable benefits of customized service delivery.

Nor does Glazer satisfactorily explain why existing programs failed (in his terms). He argues that the manipulation of employment incentives, best represented in programs like the Family Assistance Plan and the Program for Better Jobs and Income, simply cannot work (or work well) where the *obligation* to work is valued. Of course, neither of these was ever adopted. Glazer's explanation can only be sustained if the structure of existing programs reflects the same sort of underlying orientation toward social engineering. But this undoubtedly credits policy analysts with too much influence over welfare reform, for which political explanations are far more important. In other words, the failure may be due to insufficient political will to do all that is necessary and not to how reforms were initially conceived.

Schorr makes this point especially well in his chapter on health policy, where he traces the complex interplay of political and economic forces that has produced a medical care system "like an idiot savant—brilliant in limited ways (in technology, in the skill of its best practitioners) but unable to control its own direction or to be a responsible servant of the public interest" (p. 176). This outcome was surely not engineered, though it is easily understood as a political outcome now so deeply entrenched that the system seems to defy reform. Yet Schorr believes both professionals and their middle-class clients may become so dissatisfied with problems of cost, access, and regulation that pressure might build for a national health insurance program or even a national health service.

Should this happen, both authors might ultimately be vindicated. Decent health policies might then be seen to arise not so much out of individuals' sense of decency toward others as out of concern for themselves, their families, friends, and associates. The same may not be true in other policy areas, however, where the absence of political motivation may perpetuate programs that are both indecent and corrosive of social bonds.

Russell L. Hanson

Indiana University

Bifurcated Politics: Evolution and Reform in the National Party Convention. By Byron E. Shafer. Cambridge: Harvard University Press, 1988. 391p. $27.50.

The classic *Politics of National Party Conventions* by Paul T. David, Ralph M. Goldman, and Richard C. Bain (1960), is now almost three decades old. The conventions as political institutions have been transformed remarkably in the intervening years. Most obviously, they no longer perform their original function of choosing the parties' presidential nominees; instead, they now ratify as a matter of course the candidates foisted upon them by millions of ordinary (or not so ordinary) citizens participating in party caucuses and primaries.

Have the conventions therefore become irrelevant? Arguments that they have not, typically focus on their continuing importance as "launching pads" for the fall campaign. Byron Shafer's new book echoes this theme but also offers a different and more original justification for a full-dress study of the modern conventions. Shafer argues that the conventions, uniquely self-contained in time and space but nevertheless buffeted by major social trends and institutional reforms, are "a potentially rich source of insights on the political world outside" (p. 3).

In particular, Shafer claims that the conventions are a key exemplar of a more general trend toward what he calls "bifurcated politics":

> That politics still centered on political institutions, though less to build coalitions directly and then bargain over specific outcomes and more to publicize issues and build amorphous but larger external coalitions. It was these coalitions which could subsequently rebound on the organs of government, always with unpredictable timing, often with remarkable force. In any case, this process of building coalitions on two levels was the essence of a politics hovering more in the ether around institutions than in the detailed and deliberate machinations within them. (p. 4)

Eventually it becomes clear that "bifurcated politics" involves a "transition from an older era of brokerage and bargaining to a newer era of publicity and communication" (p. 324). The analogy between the convention setting and the broader political environment rests on the fact that "the same tendency toward harnessing any available institution—any available piece of an institution, really—to the raising of issues and the publicizing of causes" is "an increasingly central feature" of both (p. 310).

In the case of the conventions, Shafer argues that the networks' abandonment of "gavel-to-gavel" coverage precipitated "the bifurcated convention, with one convention on site and another (via television) at home, each powerfully different one from the other" (p. 297). As an example of the divergence between these two conventions Shafer cites the media frenzy surrounding the Ford-for-vice-president boomlet at the 1980 Republican convention. "It was quite possible," he writes, "for those in the hall to be unaware of this development almost in its entirety—and for it thus to be close to a complete 'nonevent' " (p. 283).

Here, I think, Shafer's fascination with the convention "in the hall" as a self-contained political institution leads him astray. The problem was not that the media were covering a "nonevent." The event consumed the attention of key convention participants—those at the top of the Reagan campaign—at least as fully as it consumed the attention of network anchors. If there was genuine "brokering and bargaining" that night, it went on between the Ford and Reagan staffs. The real "nonevent" was in the convention hall, where the delegates went irrelevantly about their business.

Shafer himself provides ample evidence of the withering away of the convention "in the hall." In 1988, if anything, that process accelerated. The news media cut back even further on coverage of the convention as a party gathering while focusing even more heavily on the self-presentations of the nominees (especially on their acceptance speeches). Meanwhile, even the Democrats have apparently learned how to abet this development by keeping their platform deliberations, their organized interest groups, and indeed their delegates firmly in the background.

Ironically, then, the very trends Shafer describes so well make the gaps in his "aspiringly comprehensive account" of the modern convention (p. 3) loom larger than they otherwise might. When he argues that the convention's major function now is to launch the party's general election campaign, readers are likely to be disappointed that his pursuit of convention effects into the electorate begins and ends with a comparison of pre- and postconvention polls. When he assigns considerable (and increasing) importance to the activities of organized interest groups in the delegate selection process, it seems unfortunate that the actual activities of these groups are observed only incidentally and very indirectly (for example, in comparisons of the percentages of National Education Association members among delegates selected in primaries and in caucuses).

On questions of procedural reform Shafer is noncommittal. He points out that the conventions, if left to themselves, will sooner or later be called upon to make a real nomination; but he provides few indications of how they would perform that function and indeed suggests that having performed it once, they might well find themselves reformed out of business as a result.

Generally speaking, Shafer is at his best when analyzing the concrete conflicts of interest that permeate the modern convention—conflicts between party leaders and followers, between winners and losers, and between convention participants and the news media. Read simply as an analysis of these conflicts, his is the best study of the conventions we are likely to have for some time to come. If that seems insufficient, it is because Shafer himself has upped the ante. By placing the transformation of the party conventions in a broader political perspective, he succeeds primarily in convincing the reader that the traditional convention has become so anomalous and inconsequential that it no longer satisfies what Shafer (p. 3) calls a "minimum requirement" for successful institutional analysis: that the institution be "procedurally central to major aspects of American politics."

LARRY M. BARTELS

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Political Ethics and Public Office. By Dennis F. Thompson. Cambridge: Harvard University Press, 1987. 263p. $25.00.

In this volume a political philosopher who is also director of Harvard's Program in Ethics and the Professions seeks to elucidate ethical problems faced by officials through a mixture of theoretical analysis and practical prescription. Of its seven chapters, some are more analytical and philosophical in nature, others have a more applied bent in discussing some aspects of recent U.S. experiences. What unites them is Thompson's conviction that "a political ethics of public office is necessary because so much of modern politics is mediated politics," requiring efforts like his "to preserve the essentials of the traditional idea of personal responsibility against the pressures of organizational life."

Watergate is mentioned only a few times, and the volume differs from most of the literature produced in the aftermath of that event. It is more theoretical in how it applies philosophical modes of analysis to actors in a variety of situations. It is more catholic in the sense that Thompson also draws widely on legal, public administration, and political science literatures. Finally, it deals less with the dilemmas faced by the prince or other top-level rulers and more with those faced by officials in the second or third echelons.

Themes familiar from earlier literature on morality in politics are developed in the chapter, "Democratic Dirty Hands," where Sartre's metaphor is applied to a broad range of situations in which politicians have cheated and lied. He takes issue with those like Walzer and Donagan, who would excuse immoral deals in self-defense so that it would be acceptable to defend oneself by corruption against corruption already initiated by others. For Thompson the costs of staying in the game can get too high, and he sees Cyrus Vance's resignation after lying to the United States' allies as a "model of how a dirty-handed politician should act." As for politicians who are on the take, Thompson thinks that undercover operations like ABSCAM come closer to creating than to catching criminals and even considers broadening prohibitions against large classes of covert government operations.

In a chapter, "Many Hands," Thompson deals with the problem that the very nature of bureaucratic policy making obstructs accountability. He seeks to dismantle rationales that would allow officials to lose the trace of their decisions in hierarchical and collective structures and insists that personal responsibility attaches to persons, not offices, and follows officials wherever they go.

What are the results of the paradox that although public office, as a trust, imposes higher standards of conduct than citizenship, the criminal law hardly takes note of any special duties of public office? Thompson deals with this problematic in a lively chapter that takes note of such legislation as the Ethics in Government Act but remarks obliquely that the notion of office as trust "has reappeared in American law in recent years under the curious aegis of the federal mail fraud statute." He does not dismiss out of hand the objections of those who fear that implementing legal concepts of trust could discourage robust political activity and overcriminalize the political process. But he also is not afraid to suggest new legal sanctions, for example, for officials who conceal important information from the public.

The last four chapters are more disparate. One on legislative ethics selects a number of themes and spells out what Kant or Rawls might have to say on those issues. Of two that are narrower in scope, one dealing with private lives of officials examines cases like those of the congressmen who were publicly pilloried for sexual involvements. We are told that those who engaged their fancies with *publicly paid* assistants were fair game for publicity but that attacks or an affair with an *unpaid* campaign assistant constituted an invasion of privacy. The other deals with some issues in compensation adjustment for families that were "disenrolled" in the aftermath of a flawed federal income maintenance experiment.

While Thompson's conceptual apparatus

does not attempt to conform to particularly fashionable paradigms, the only essay that might appear conceptually dated is "Paternalistic Power." It contains some interesting sections, like the discussion of whether professional paternalism is more pernicious when practiced by doctors or by lawyers. But the variety of problem situations cursorily dealt with causes the treatment to appear somewhat rambling and insufficiently tied together by the umbrella concept, as well as being somewhat removed from the main themes of the book.

Overall, the volume constitutes a serious attempt to deal with questions that have remained underexplored in the interstices between academic (demarcations), and it should appeal to those seeking to interrelate normative and empirical approaches to politics.

ARNOLD J. HEIDENHEIMER

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The Rhetorical Presidency. By Jeffrey K. Tulis. Princeton: Princeton University Press, 1987. 209p. $19.95.

This book is a valuable addition to the substantial literature that examines the relationship between the presidency and the public. Tulis utilizes a data set consisting of presidential communications to Congress and the nation, from George Washington through Ronald Reagan, and subjects it to content analysis. Tulis's central argument is that the "rhetorical presidency *does* represent a true transformation of the presidency" (p. 7). His purpose is to demonstrate that the marked distinction between presidents' rhetorical practices in the nineteenth, as opposed to the twentieth, century reflects alternative understandings of the Constitution and different conceptions of political leadership. He maintains that these differences are a major cause of the "dilemmas of modern governance" (p. 17).

Tulis presents a tightly reasoned case that moves from the intentions of the founders to the consequences of contemporary presidential rhetoric. According to Tulis, the founders sought to prevent demagoguery and establish representative, not popular, government through the independence of the executive and the separation of powers. In order for constitutional theory to work, presidents, members of Congress, and judges were required to behave in ways "consonant with special tasks and perspectives of each governmental branch" (p. 45). Thus, nineteenth century presidential rhetoric reflected constitutional theory in two major ways: policy speech was to be written, and communications with the people were to be consistent with the fear of demagoguery and the commitment to republicanism. Communications with the people tended to be few in number and vague, general, and innocuous in content. The only pre-twentieth-century president who deviated from the prescribed rhetorical norms was Andrew Johnson—bad and improper rhetoric was one of the impeachment charges against him.

Theodore Roosevelt dramatically altered rhetorical practice with speaking campaigns in support of his policies. His oratorical success demonstrated the potential of popular leadership, but he justified it as a means of overcoming demagogues (other than presidents) and restoring deliberative republican governance. Woodrow Wilson established the modern form of presidential rhetoric and reinterpreted constitutional theory to justify it. He regarded separation of powers as the principal defect in the Constitution. While the founders regarded popular leadership and deliberation as antithetical, Wilson maintained that they complemented each other. Where the founders believed that republicanism required some insulation between the people and their representatives, Wilson viewed publicity as "the very heart of representative government" (p. 127). The president's national constituency and his independence require that he be a leader who interprets the public will. Under Wilson's reformulated theory, presidential authority comes directly from the people, not from the Constitution. For Wilson, "the need for more energy in the political system is greater than the risk incurred through the possibility of demagoguery. This," Tulis concludes, "represents a major shift, indeed a reversal, of the founding perspective" (p. 132).

Tulis's content analysis of nineteenth- and twentieth-century presidential speeches reveals the new Wilsonian norms. The problem that the modern rhetorical presidency poses is that there are limits to popular leadership imposed by the logic of the Constitution, which remains unchanged. The two theories of the Constitution—the founders' and Wilson's—"do not fit together to form a consistent whole. Instead, elements of the old and new ways frustrate or subvert each other" (p. 146). Successful employment of presidential rhetoric can lead to a loss of presidential credibility (as Wilson experienced in his campaign to force Senate approval of the League of Nations covenant) and to breakdown in the deliberative process as a whole (as occurred after Lyndon Johnson's War on Poverty campaign).

Tulis, then, does not regard the rise of the modern rhetorical presidency as a logical and benign development of the office. For him, the rhetorical presidency represents a malignant transformation of the institution into a schizoid hybrid whose constitutional structure embodies the founding doctrine but whose procedural norms require conduct responsive to popular expectations in a way that is at odds with constitutional logic. The result is serious problems for "modern constitutional rule" (p. 174).

Tulis's solution to the dilemma of presidential leadership is to return to original constitutional theory and nineteenth-century rhetorical practices except in crisis situations. What is needed is "a new political education" leading to a reformed polity in which citizens can "judge the rhetorical categories" of power and presidents can "avail themselves of . . . rhetorical examples of principle, vision, and silence" (p. 204).

The persuasiveness of Tulis's argument turns on the equation he posits between presidential rhetorical practice and constitutional theory. Is presidential rhetoric the best—or even an accurate and complete—reflection of such theory? Are there not other sources of it, such as judicial opinions, statutes, congressional debates and committee reports, the speeches of other political leaders, and scholarly analyses?

This is an important book that directs attention to the dilemmas that the modern public presidency, as presented and manifested in presidential speeches and addresses, poses for constitutional government. For Tulis the problem is a reformulated constitutional theory so at odds with the objectives and design of the founders that the office has become ambiguous and republican governance is imperiled. According to Tulis, reformist tinkering with the office is not the solution. He recognizes that only presidents are in a position to set things right. The difficulty is that presidents are trapped by rhetorical practices—based on popular expectations—that are of great short-run political benefit to them. The need is to elect presidents who are willing to modify those practices in the long-run interest of enhanced republican governance. Tulis would have such presidents reeducate the citizens regarding fundamental constitutional principles. He advocates a less-visible and scaled-down linkage between the president and the people. My belief is that the forces that have created the modern public-plebiscitary presidency revealed through Tulis's analysis of their rhetorical practices are too powerful to be turned aside, even by carefully reasoned scholarly arguments such as his. Neustadt's prescription for successful presidential leadership based on persuasion remains the most useful guide for making a difficult constitutional system tolerably workable. Tulis, through his insightful analysis, helps us to understand why that system has become so frustrating.

NORMAN C. THOMAS

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Social Structures: A Network Approach. Edited by Barry Wellman and S. D. Berkowitz. New York: Cambridge University Press, 1988. 513p. $29.50.

This excellent collection of 18 essays on structural and network approaches to social phenomena comes to us from sociology, and I recommend the book to all who want to know what is going on at probably one of the most exciting "cutting edges" of another discipline, all the more so because an earlier trend (in the 1950s and early 1960s) toward a rapprochement between political science and sociology seems to have come to a halt (unfortunately in view of the fact that they share such eminent nineteenth- and early twentieth-century figures as Comte, Durkheim, Gierke, Marx, Maine, Maitland, Simmel, Spencer, Weber, and others). But I recommend this book especially to those not totally smitten by the current vogue to explain "collective action" through the individualistic calculus of "economic man" or by the rediscovery of "the state" as a presumably useful unit of systemic analysis. It seems to me that neither the methodological individualism of the former mode of social inquiry nor the methodological holism of the latter are sufficient to cope with the enormous complexity of social and political phenomena (though I will concede that rational choice models of collective action constitute one of the few serious attempts in social science to link micro and macro levels of analysis). Structural or social network approaches surely deserve more attention from political scientists than they are being given.

Although the collection's subtitle speaks of a network approach, there are, in fact, several "approaches"; and this should make the book particularly attractive to those who wish to be initiated into relevant theories and methods. Moreover, the collection covers a wide range of substantive concerns that the editors have organized under the rubrics of "communities," "markets," "social change," and "social mobility," though others might have been chosen. Particular chapter titles show that there are topics of interest to political scientists that fall into our own conventional specializations. I can only give some examples, but there is something on political behavior ("Networks As Personal Communities"), elites ("Collective Mobility and the Persistence of Dynasties"), comparative politics ("Relations of Production and Class Rule"), international relations ("Form and Substance in the Analysis of the World Economy"), and even (Marxist) political theory ("Structural Location and Ideological Divergence"). If I don't mention their authors, it is because their names are hardly household words in our discipline. I might single out Harrison C. White, whose "Varieties of Markets" is not only a spirited critique of the microeconomic approaches to market behavior (with their overemphasis on cognitive psychologism) but also makes a persuasive argument for what in network lingo is called "blockmodeling." Taking a crack at the almost exclusive reliance of microeconomics on statistical techniques and strategies of data analysis, he writes, "When suitable labels are applied to common statistical terms, many social scientists deceive themselves into believing that some theory is being applied" (p. 232).

Though the term *network* is not unknown in political science (some of us may even remember Robert McIver's "web of government" as an alternative), it is more often than not used only as a convenient metaphor (e.g., "policy networks"). This the term is decidedly *not* in its current sociological usage. Ever since English social anthropologists began the study of social structures as networks in the 1950s, U.S. and Canadian sociologists, working in the sociometric tradition, have given the term and what it stands for rigorous theoretical and methodical definition and application. Four chapters under the rubric "Thinking Structurally" are useful introductions for the novice. Perhaps the most important point I should make about all this is that networking as theory and method is a *generic* set of approaches to social analysis, applicable not only to interpersonal relations but also to intergroup relations as well as relations within and among such relatively amorphous collectivities as social classes and movements, ethnic and religious groupings, "crowds," and what are called "communities of fate" (e.g., political refugees or migrant workers). As several chapters here demonstrate, network analysis can bring exceptional analytic rigor to social phenomena that without it appear to be more labyrinthine than is necessary. In short, it seems to me that "network thinking" forces us to be more sensitive than we normally are to such theoretical and methodological issues as the identification of units and specification of levels of analysis (which are often confounded), the choice of units and levels of analysis, and the properties of the units that serve as variables in either quantitative or qualitative analysis. Network analysis of one kind or another thus goes a long way not only in closing what is called the "micro-macro gap" but also in sensitizing us to the complex relationship between "parts and whole" that continues to baffle the study of politics.

The editors have done an outstanding job of editing, in both the technical and intellectual sense. Their introductory and closing chapters, as well as short introductions to the collection's five main parts, pull the many diverse strands of network thinking and the particular contributions to the volume into a whole equally sensitive to what is integral to "structure" and to what is partial. All that is needed now is a network of potential readers who might constitute that mythical grouping called "invisible college."

HEINZ EULAU

Stanford University

Papers on the Ethics of Administration. Edited by N. Dale Wright. Provo, UT: Brigham Young University, 1988. 252p. $39.50 cloth, $12.95 paper.

In 1987 a group of scholars met at Brigham Young University to discuss organizational ethics. This book is a collection of the essays presented at the meeting.

Despite a focus in the introductory essay on both public sector and business organizations, the majority of the nine essays are concerned with ethics in the corporate world. The public sector exceptions are John Rohr's "Reason of State As Political Morality" and F. Neil Brady's "Ethical Theory and Public Service," which relies heavily on an earlier work by Rohr on bureaucratic ethics. A third paper, David L. Norton's intriguing " 'Character Ethics' and Organizational Life" alone presents a purely philosophical approach to organizational ethics.

This is a very uneven collection, made confusing by a multiplicity of definitions and approaches. The authors draw from different and often incompatible academic traditions and literatures. The volume would have been strengthened considerably by a series of editorial comments on each essay. Several of these essays are valuable as freestanding academic arguments, but many of the contributions are primarily lamentations on the sorry state of corporate ethics.

N. Dale Wright and Stanford McConkie, who wrote the introduction, note correctly that all the papers return to the theme that "organizational ethics is dependent upon personal ethics" (p. 18) and that they all are concerned with the changing discipline of ethics. However, nowhere do the editors—or indeed the authors—attempt to grapple with the very real differences often observed between public sector ethics and business ethics. While the differences may be inferred, as in David K. Hart's discussion of benevolence and virtue as a counterpoint to self interest, the entire volume rests on the assumption that organizations are sufficiently alike that one may discuss the ethics of all when discussing the ethics of any.

While several authors lament the gap between personal and organizational ethics, only Rohr explicitly addresses the normative aspects of this distinction. Relying on Niebuhr's observation that "collective morality is *inferior* to personal morality" (p. 186), Rohr argues that compelling reasons exist for the dichotomy between our inner sense of morality and our obligations as public servants in the U.S. polity.

Most troubling in this collection of essays is the instrumental theme underlying the papers on corporate ethics. Probably the most blatant exposition of this is found in Margaret J. Wheatley's "The Motivating Power of Ethics in Times of Corporate Confusion." Her intention is to show how a strong sense of personal ethics and responsibility to subordinates can sustain an administrator when the organization is itself in disarray. However, her discussion concludes that the primary benefit of this behavior is not the personal well-being of the employees but rather the continued productivity of their corporate units: "Ethics can motivate people to transcend organizational circumstances and continue to be productive contributors" (p. 142). Norman Bowie, in "The Paradox of Profit," has tried to avoid this pitfall by discussing how corporations can do *well* by doing *good*, but the specter of the profit-driven bottom line lurks throughout the business-oriented papers.

The most provocative essay in the collection (and thus the most useful for pedagogical purposes) is David Norton's " 'Character Ethics' and Organizational Life." He distinguishes between the classical study of ethics as character-forming and the modern approach, following Hobbes, Kant, and Rawls, of ethics as rules. In this essay he provides a convincing discussion of the basis for the U.S. concept of value-neutral administration and outlines some implementation strategies to remedy the alienation often found in bureaucratic organizations. He writes what may well be the last word on attempts (such as this volume's) to reconcile ethical behavior in an individualistic corporate environment: "The recently mushrooming interest in 'business ethics' will do nothing to open up prevailing conceptions of moral theory and practice to larger human aspirations, since it is but the application to business situations of minimalist rules morality" (p. 55).

The discussions of business ethics, while well intentioned, serve to illustrate the chasm existing between instrumental approaches to ethics as a source of motivation for productivity and normative approaches from philosophy and political theory. The collection is an encouraging effort to instigate discussion between the two fields. Certainly, the concept of *organizational ethics* provides a tenuous semantic bridge. However, the disciplines of business administration and public administration are sufficiently divergent that more than simply assembling scholars is necessary for convincing dialogue. Basic assumptions about the purposes of organizations and the role of individuals in society must be uncovered and agreed upon before launching any meaningful discussion of organization ethics.

SUSAN J. BUCK

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COMPARATIVE POLITICS

The Military and Politics in Nkrumah's Ghana. By Simon Baynham. Boulder: Westview, 1988. 294p. $36.50 paper.

Baynham's book is an important, empiric, analytic, and social-political-ethnic study of the interplay of civilian-military relationships in postindependence Ghana from 1957 to 1966. The author views Ghana's military as a pivotal institution and underscores the primacy of politics in Ghana. He describes the Ghanaian politics as "a perennial struggle by individuals and groups to maintain and extend the economic standing by gaining control over the allocative powers of the state." He asserts, "Political office is the main source of power and the key to the preservation or improvement of personal and corporate living standards" (pp. 8–9).

The salient themes of the book are the composition and structure of the army as measured against local indexes of social hierarchy, the rate and impact of institutional transfer of power and the effect of indigenization on the organizational stability of the military establishment, and some historical analysis and interpretation of the relationship between Nkrumah's Convention People's party regime and the modern military forces in Ghana.

The study focuses on Nkrumah's years as a leader of Ghana. His personal rule, political intrigues, repeated plots on his life, schisms, purges and the deep-seated political-regional-ethnic conflicts, divisions and tensions inherent in the Ghanaian body politic, and the tactics used by Nkrumah to contain the destructive opposition are among the revealing and telling sections of the book.

The army in the postindependence Ghana was the preserve of the British. Training, military equipment, policies, standards, and almost all expatriate personnel were British. It was this factor which compelled Nkrumah to quicken the pace of indigenization, expansion, and diversification processes in the military. Baynham observes, "For several years after independence, the Ghanaian Army continued to rely almost exclusively upon the former colonial master" (p. 66).

Succinct discussions deal with the Ghanaian forces' intervention in the 1960 Congo crisis. But Ghana's involvement was a challenge and test of the mettle, skill, and effectiveness of its army. Above all, it confronted Nkrumah with irreconciliable contradictions and dilemmas, which greatly embarrassed him in the eyes of other African leaders. The Ghanaian military forces were dominated by the British army officers during the Congo operation. How could Nkrumah play an effective leadership role in African affairs when his own army was still commanded by a large number of clearly recognizable British officers? Pragmatically, Nkrumah reasoned that the slow pace of the British Africanization plan and the British government help to the insurgent factions in Katanga made it "politically imperative that . . . direct command of the Ghana Armed Forces should be held by Ghanaians" (p. 109). Hence, his decision to dismiss the British military officers in September of 1961 must be seen in the light of this circumstance.

Nkrumah's institution of the Guard Regiment as a security measure, the conflicts and rivalries between the guard and the regular forces, and his suspicion about the loyalty of the army raise some of the most intricate questions. His buildup of the alternative security services and his militarization of civilian organizations were inferred by the army as a "plan to build a second army" and an attempt "to strangle the Regular Army to death" (p. 142). Therefore, the fear of being squeezed out of existence united the army against him. Another significant factor was the domino effect—the spate of military coups occurring in other African states. The Nigerian coup in January of 1966 was a catalyst. Following that rebellion, several army officers sustained the hope *our turn next*. Hence, the putsch that toppled the First Republic of Ghana on 23-24 February 1966 was a culmination of several months and years of intensive clandestine plannings. The nine Ewe officers who led the coup were afraid that they would be among the first victims of Nkrumah's move against the military. The author emphasizes the crucial role played by the police in the ouster of Nkrumah.

The author aptly observes that "the military leadership behaved very much like the civilian politicians they replaced, thereby directly contributing to, rather than altering, the prevailing economic, material, and status basis of Ghanaian politics" (p. 227). Certainly, the incessant military upheavals in post-Nkrumah Ghana—the Acheampong (1972), Akuffo (1978), and Rawlings (1981)—dramatize the instability, vulnerability, and contagiousness of military intervention.

This is a readable, well-written and well-documented book. Baynham is painstaking, realistic, and objective in his assessment of the tangled web of civilian-military relationships in Ghana. His approach is informative, arresting, and enlightening. His repeated use of the words *tribe* and *backward*, however, is inappropriate. Most African writers today do not use such words, due to their perjorative connotation. Furthermore, his imputation of inordinate and personal ambition to Nkrumah's stance on the issues of African liberation and unity is erroneous. But within these limits, his book has added new dimensions to our understanding of the saga of military politics in Africa.

SMART A. EKPO

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Restructuring Domination: Industrialists and the State in Ecuador. By Catherine M. Conaghan. Pittsburgh: University of Pittsburgh Press, 1988. 197p. $28.95.

The chasm between comparativists specializing in the developing areas and scholars putatively lodged at or near the mainstream of political science orthodoxy has widened perceptibly in recent years. Research of those in the former grouping increasingly evidences unconcern over this course of events, alternatively scorning, or denying the need for, broad disciplinary focuses. Happily, such is not the case with *Restructuring Domination*, for Catherine M. Conaghan has produced a work of modest length that nonetheless addresses important intellectual issues. Her particular subject centers on the comparative analysis of industrial class politics in Latin America and is explicitly linked to literature extant for the industrial bourgeoisie in general.

Material illustrative of the author's theoretical interests is effectively provided by the case of Ecuador. Some two-thirds of the book sketches the evolution of its industrial bourgeoisie, carefully noting determinedly successful efforts in recent years to assure the failure of reformism in the small Andean nation. During a self-consciously "nationalistic" military regime introduced in 1976, Ecuador's dominant classes made apparent both their systematic rejection of change and an unbending recalcitrance toward conditions of uncertain access to policy makers. This approached the point of irrationality under subsequent civilian rule during which centrist presidents adopted economic policies eminently favorable to the industrial bourgeoisie. The perception of limited access proved more powerful than the actual content of policy.

An important source of original empirical data is Conaghan's eight interviews with Ecuadorean business executives in 1979 and 1980. As detailed in an appendix useful for novices at field research in Latin America, this open-ended probing of industrialists' attitudes was supplemented by discussions with relevant government policy makers, as well as richly varied sources of printed information on major Ecuadorean firms. The timing of the interviews mitigates against analysis of industrialists' interactions with the state during the fiercly aggressive probusiness administration of Leon

Febres Cordero (1984-88). This could have provided further insight into the pattern of attitudes and interrelationships that has crystallized with the exploitation of Amazonian petroleum and the concomitant transformation of the state since 1972. At the same time, the study under review is not designed to be a politico-economic narrative. Those in search of such a work might profitably explore the pages of David Schodt's perceptive *Ecuador: An Andean Enigma* (1987).

Given the primacy of the author's conceptual preoccupations, she properly engages in comparative inquiry by devoting space to the historical experience of industrialists in Chile, Argentina, and Uruguay. In these nations there were significant developments early in the twentieth century, as horizontal import substitution encouraged a form of industrialization that was accompanied by political participation and by acceptance, however begrudging, of measured social change. Conaghan contrasts these industrial bourgeoisies with that of Ecuador, where a small and limited internal market has continuously diluted any potential interests in local reforms. The differences are stark, dramatizing effectively the folly of overgeneralizing about Latin America.

For Ecuador, the bourgeoisie emerged from a late-starting process of industrialization. Consequently, its circumstances are substantially dissimilar from earlier Southern Cone counterparts. Even the petroleum bonanza failed to provide stimulus for change or reform. The industrialists continued to rely on the external market and heightened state subsidization, while the limited internal market was somehow of minimal relevance. Thus, while horizontal import substitution elsewhere in Latin America was stirred and affected by populist reformism, the Ecuadorean case demonstrates "how economic structure and the immediate political conjuncture can heighten the bourgeoisie's capacity to resist reform and restructure the political system so as to remove reform from the public agenda" (p. 13).

A brief concluding chapter on the transition from military reformism to democratic authoritarianism in Latin America whets the reader's appetite for extended comparative discussion of industrialists and their role in that crucial process. If that hunger is not satisfied here by Conaghan's brief "reflections" on industrialists' political responses to reformism, her study paves the way for future research. Her projected work on the Peruvian and Bolivian experiences promise further to advance our understanding. In the meantime, this work has made a fruitful contribution in its own right.

JOHN D. MARTZ

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Socialism's Dilemmas: State and Society in the Soviet Bloc. By Walter D. Connor. New York: Columbia University Press, 1988. 299p. $32.00.

Is Russia Reformable? Change and Resistance from Stalin to Gorbachev. By Robert V. Daniels. Boulder: Westview, 1988. 141p. $28.95.

The two books reviewed here complement one another both in form and content. Both represent collections of essays, most of which were previously published elsewhere. Both treat general problems of social and political change in the USSR (and, in Connor's book, Eastern Europe as well), analyzing the reform agenda in historical perspective. The difference in their points of departure reflects complementary outlooks on change. Whereas Connor stresses the transformation of the social structure of the USSR and Eastern Europe since the war, giving particular attention to changes in the composition and outlook of workers, Daniels emphasizes elite politics and ascribes central importance to the wholesale replacement of the Stalin generation in the Soviet leadership with cohorts less insular and conservative.

Connor's essays illuminate several facets of the relationship of modernization to class mobility. The enormous expansion of educational opportunity and employment in skilled manual, supervisory, and technical jobs in the first generation of "socialist construction" tended to contribute to social stability despite the lowering of average living standards, the harsh and coercive regimentation of society, and the traumatic dislocations that rapid industrialization produced. The progressive decline in opportunities for social mobility since then has tended to sharpen class distinctions by enabling a hereditary working class to form. As complete secondary education became nearly universal, and the structure of the job market changed only slowly, society grew increasingly incapable of satisfying the aspirations of the children of the manual working class and therefore reinforced differential advantages enjoyed by different strata in access to higher education. Connor argues that the Soviet regime has opted to restrict intake to postsecondary educational establishments precisely in order to avoid facing "an overlarge, underemployed 'intelligentsia,' its appetite whetted for social, economic, and influence/power rewards the system cannot deliver" (p. 94). In fact, however, this seems to be exactly what has resulted from the steady expansion of postsecondary educational opportunity: enrollments in higher and specialized secondary institutions more than doubled from 1960 to 1975. As a result, more and more specialists find themselves performing manual or other jobs well below their level of training. As one recent Soviet analyst put it, this problem "in fact has led to the dequalification of many engineers—specialists with higher education—who are forced to perform the functions of a technician, a foreman, a lab assistant, etc." In many ways, the Gorbachev regime's challenge has been to respond to the grievances of an overlarge, underemployed intelligentsia without at the same time aggravating relations with the working class.

With respect to the contemporary period, the question Connor addresses at several points concerns the prospects for a common front of workers and intellectuals. In an essay that originally appeared in January 1980, Connor is generally doubtful about the likelihood such an alliance might form, or about the capacity of workers to sustain an independent organization for resistance against the state. In view of the emergence of Solidarity only months after this article first was published, Connor is brave to reprint it without revision. Still, as he observes, Poland's experience was not typical and similar social movements seem to be far from coalescing elsewhere in the bloc. On the basis of various kinds of evidence, including the writings of unorthodox East European intellectuals and opinion surveys carried out on behalf of Radio Free Europe, he discusses shifts in public political opinion, finding a widespread desire throughout Eastern Europe for relaxed East-West relations as the only political environment that can permit the development of a measure of national cultural autonomy. In an insightful concluding essay, Connor finds among intellectuals everywhere except in the German Democratic Republic a movement toward what might broadly be called a social democratic rather than a utopian-Marxist position, such as had been characteristic in the first decade or so of socialism; only in East Germany has belief in a purified version of Marxism remained strong among the intelligentsia.

Daniels's essays are more disparate in theme and treatment. Two are devoted to the Soviet intelligentsia, one to the collapse of the liberal period of the late 1950s and early 1960s, the other to the period between the mid-1960s and mid-1980s, most of which was a dark age of conformism and mediocrity until Gorbachev emerged as leader and issued an invitation to the intelligentsia to help him reconstruct socialism along more liberal and democratic lines. Daniels emphasizes two themes in these essays, one cultural and one structural: the resilience of certain norms in the political culture of autocratic Russia, such as fear that open disagreements might be exploited by the state's enemies; and the "circular flow of power," a term he first introduced (it has subsequently been given wide currency) to explain the process of power consolidation of a leader's position. In his chapter on generational replacement, Daniels provides a succinct overview of this mechanism and uses it to help explain how the Stalin generation maintained its grip on power until the 1970s and even into the 1980s.

Taken as a whole, however, the book represents more a collection of commentaries than a sustained examination of a particular set of problems. Little original research is represented here. And much of what Daniels has to say (e.g., about the representational function of the party Central Committee) repeats earlier work. The reader will not find here a compelling explanation for the scope and nature of Gorbachev's radical reform program and is likely to consider Daniels' sunny final chapter, foreseeing a successful outcome to the effort to restore a democratic, antibureaucratic, "truly Leninist" model of socialism, to be unduly sanguine about Gorbachev's prospects, and, for that matter, his intentions. It is one thing for the Italian Communist party to jettison obsolete and mythologized doctrines. It is quite another for a ruling Communist party—still more one ruling a multinational federal state—to abandon its ideology.

What one misses in both books is a sense of the economic forces bearing on social structure and national policy. Widely accepted among economists in both socialist and capitalist countries is the observation that the methods of forced economic modernization through "extensive" methods, (the mobilization of labor and capital and the imposition of comprehensive central planning and administration) have not merely outlived their usefulness but have become positive detriments to modernization. Each effort at reform, on the other hand, creates undesirable and often uncontrollable outcomes manifested in regional inequalities, inflation, shortages, profiteering, and a widespread loss of confidence in the leadership on the part of those least well positioned to take advantage of competitive opportunity. These are the political and social realities with which virtually all communist leaderships in the 1980s have struggled, and there is every indication that the tensions they produce will grow still more acute in the 1990s.

THOMAS F. REMINGTON

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Soviet Succession Struggles: Kremlinology and the Russian Question from Lenin to Gorbachev. By Anthony D'Agostino. Boston: Allen & Unwin, 1988. 274p. $45.00.

Part of the recent turn toward revisionist historiography in Soviet studies, D'Agostino's work provides a spirited defense of the study of leadership conflict (labeled "Kremlinology"). (Less clear is his defense of Kremlinology *against* what he somehow sees as the alternative, grand ideological formulations raised by questioning the nature of the Soviet state and society—the "Russian Question.") This is an ambitious book that seeks to change not only our terminology but also our understanding of the processes and policy content inherent in leadership factionalism over seven decades of Soviet history. In this sense D'Agostino's book may seek to take its place alongside Stephen Cohen's study of Bukharin—even offering a Zinoviev thesis as corrective. Yet in the end this book may not succeed.

Central to D'Agostino's interpretation are the concepts of centrism and the "alternance" of policies between a Moscow platform and a Leningrad platform. The former program urges "solicitude for agriculture, assurance for the denizens of the state machine," a policy of foreign participation in the economy, and detente in foreign policy. The latter promotes the primacy of industry and planning, autarky, and revolutionary internationalism and militancy. Leaders rigidly attached to either platform have enjoyed neither power nor recognition. Conversely, the leaders who have guided Soviet destinies over seven decades—Lenin, Stalin, and Suslov [*sic*]—are those who have occupied the center between these platforms, executing turns from one to its opposite.

The thesis is provocative, but the author's explication of its key elements leaves much unclear. Missing from the account is an explanation of the sources of the dynamic that make alternance and centrism constants of factionalism. Moreover, the author seems to dispute his own thesis when the meanings of both alternance and centrism appear to change over time—particularly under Suslov. And alternance seems virtually to disappear under Brezhnevian collective leadership.

The existence of alternative Moscow and Leningrad platforms—in particular, the coherence of the policies assigned to each platform—is less than convincing. And the continuity of policy content over time seems to be at odds with the author's assertion that leaders (including the purported partisans of these platforms) shift their views with considerable ease. The attempt to find continuity and assign leaders to one platform or the other, particularly in the post-Stalinist years, may lead the author to misrepresent the positions of many. The notion that Stalin's success owed much to his centrism is an important one. One wonders why the author did not choose to extend this thesis to explain the success of Khrushchev, Brezhnev, and their heirs rather than rely upon Suslov ex machina to save the plot. The dubious contention that Suslov provided the anchoring centrism in post-Stalinist politics is simply asserted, never demonstrated. The corollary to the notion of centrism that partisans of either platform are less successful may be an important insight, yet it is undermined in the author's explication by the tendency of even the so-called partisans to alter their views with seeming alacrity.

The importance assigned foreign policy and issues of international communism in Soviet factionalism, particularly after the twenties, is never defended. Indeed, this theme is developed primarily by ignoring domestic socioeconomic developments. In this sense, a study such as George Breslauer's *Khrushchev and Brezhnev As Leaders* (1982) may provide a necessary balance to D'Agostino's treatment of the issues in Soviet leadership politics.

The most enduring contribution of this work may be its caveat that the post-Lenin succession was a more complex and fluid process than would be suggested by subsequent interpretations that focus on Trotsky and Stalin or the opposition between permanent revolution and socialism in one country. Yet in the end the reader may conclude that the author's speculative challenges to key elements of the prevailing interpretation are not enough to abandon that well-argued orthodoxy. Sadly, the attempt to extend the story beyond the twenties adds little to now-familiar stories told by emigré Kremlinologists.

Specialists will find this a rewarding book for the ideas that it suggests or provokes. This may not be a book for readers without significant grounding in Soviet history. A complex history is made even more obscure by a focus that shifts between Moscow and expatriate commentators without a clear conceptual demarcation between the two. Political scientists may be disappointed in D'Agostino's analytic absolutism that sees conflict as a constant in Soviet politics, his failure to address the interpretations of such scholars as Breslauer or Myron Rush, and his tendency to lose his major ideas in history that is often presented as a seamless web.

PHILIP G. ROEDER

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The Poverty of Communism. By Nick Eberstadt. New Brunswick: Transaction Books, 1988. 317p. $24.95.

This book is a collection of essays, all of which have previously been published. They vary widely in content, from a submission to the U.S. Congress Joint Economic Committee to a seven-page book review originally appearing in *Society.* The style is that of high-class journalism, even being polemical in character and sometimes vitriolic in tone. There is considerable repetition of the main themes and a firmer editorial hand should have been exercised to make such a collection into a proper book.

The themes are not without interest. The author makes a spirited defence of the totalitarian approach to Marxist-Leninist states and argues his points persuasively, often with considerable documentation in support. In his review of Gordon B. Smith's *Public Policy and Administration in the Soviet Union* (1981) he dismisses the 'developing nation' approach and scorns the authors who attribute pluralism to manifestations of political input in Marxist-Leninist states (with *Pluralism* and inputs in quotes). The author pats Seweryn Bialer on the back for doing his homework—*The Soviet Paradox* (1986) "is informed by an extraordinary understanding of the Soviet state"—but only to give him a really good kick up the backside for getting it wrong. Bialer is said to mistake authoritarianism for totalitarianism, trivialize the role of ideology and ignore that of propaganda, allows himself to be unwittingly duped by Soviet foreign policy intentions and gullibly taken in by the CIA's grossly mistaken underestimation of Soviet military strength. Detente for the Soviets is "simply a policy by which to make the [Soviet] quest for international power safer and surer" (p. 108).

In the more empirical parts of the book, the author turns to examine and expose the policies of communist states. Here many empirical data on the states' performance have been collected and used to substantiate the author's theme. The Marxist-Leninist state, it is contended, "presumed a fundamentally new correspondence between the power of its apparatus and the poverty of populations" (p. 2). The sacrifice of liberty has been legitimated as necessary to achieve the elimination of poverty. Eberstadt examines this claim with respect to five areas of material poverty: population, nutrition, literacy, the status of women and material or social inequalities. He focuses mainly on the USSR, China, and Cuba, though Mongolia, Vietnam, East European, and other newer Marxist-Leninist states are brought into the picture in many places. A feature of the book is that appropriate noncommunist countries are used as comparisons. The intention here is to enable readers to "judge for themselves the success of Communist regimes in supplying daily bread" (p. 10). It is concluded that statistical indicators show that Marxist-Leninist regimes "in reducing destitution may sometimes seem to correspond to . . . non-communist nations with whom comparisons would be appropriate; when it comes to inculcating prosperity, [however], their performance for the most part appears poor" (p. 308). An important conclusion running through the articles is that Marxist-Leninist states cannot be legitimated through their intention to alleviate poverty.

In evaluating Eberstadt's contribution to the study of poverty under communism, it must be recognized that following writers such as Feshbach and Davis, he has pointed to a deterioration in health conditions and to a reduction in life expectancy. His work has provided a useful corrective to the widely held view that communist states are necessarily superior to capitalist ones in combating poverty in poor countries. It is persuasively argued that development is uneven both through time and between countries. In this respect, however, it might have helped the comparisons if Marxist-Leninist states had been differentiated into revolutionary, developmental, and modernized societies and if such stages were a basis for contrast with noncommunist ones. Given the partisan stance of the author, I also doubt whether readers will be in a position to weigh up the pros and cons of the argument, as promised. To take one example, Ellen Jones and Fred Grupp's explanation that improvements in statistical reporting is one cause of an apparent rise in infant mortality is misrepresented, and in my view the attempt to refute it by reference to other secondary sources is unconvincing. The summary of Jones and Grupp's article (pp. 34–35) ignores the fact that they do concede that there was a "less dramatic rise in overall USSR infant mortality rates" ("Infant Mortality Trends in the Soviet Union," *Population and Development Review* 9 [1983]: 214). Also, republished unrevised essays become dated. In a book published in 1988 on this topic, one would have expected some mention of Mark G. Field's conclusion that "about one quarter of the rise [in infant mortality rates] was a statistical artifact," the remainder being accounted for by regional shifts in births and a deterioration in health conditions (*Advances in International Maternal and Child Health* 6 [1986]).

The totalitarian approach tends to conflate the striving of Marxist-Leninist states to exert total control with the actual division between state and society. Societal demands and resistance to state control increase considerably as these societies modernize. Consequently, the methodology adopted in these essays is less useful for the understanding of the various reform movements that currently characterize the politics of the more developed Marxist-Leninist states. Nevertheless, the book will be useful in courses of comparative politics including communist societies and also for those including discussions of the welfare state. Perhaps it might also be used by an imaginative teacher of U.S. politics to help understand how U.S. citizens view the Marxist-Leninist states.

DAVID LANE

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Open Borders? Closed Societies? The Ethical and Political Issues. Edited by Mark Gibney. New York: Greenwood, 1988. 211p. $39.95.

Latin American States and Political Refugees. By Keith W. Yundt. New York: Praeger, 1988. 236p. $37.95.

International Differences in the Labor Market Performance of Immigrants. By George J. Borjas. Kalamazoo, MI: Upjohn Institute for Employment Research, 1988. 106p. $15.95 cloth, $8.95 paper.

People have always moved across borders for the purposes of ensuring their physical security or in search of economic well-being. However, in the late twentieth century a dramatic increase in international migration of people has highlighted the issue and drawn considerable attention from students of political theory, comparative politics, and international relations. Michael Walzer's *Just and Unjust Wars* (1977) and *Spheres of Justice* (1983), Stanley Hoffmann's *Duties beyond Borders* (1981), and John Rawls's *Theory of Justice* (1971) sparked a philosophical debate between state's rights advocates and those who argue that these rights should be modified by the principle of equal moral worth of individuals. The three books under review participate in this debate even when the authors may not have any intention of doing so.

The debate forms the backdrop for *Open Borders? Closed Societies?* edited by Mark Gibney. The contributors to this excellent volume question the absoluteness of a state's right to accept or reject refugees and immigrants solely on the basis of national security and prudential concerns for economic growth. In particular, they take issue with Michael Walzer (*Spheres of Justice*), who likens a nation-state to a private club with the absolute right to decide who it may or may not admit. In place of Walzer's liberal communitarianism the essayists present "a different view of rights, in which priority is given to the equal right of all people to the basic necessities of life" (p. 121). However, the contributors stop short of advocating that Rawls's difference principle be made the basis of states' immigration and refugee policies, for they recognize that to make it so "would require the most radical changes in the world order" (p. 117). A thorough scrutiny of the immigration and refugee policies of the United States, Canada, and Australia by the authors reveal that each nation's policies fall short of the standards of justice and morality its people hold dear. Each essay advocates a reassessment of the Western nations' response to the issue of immigration and refugees and highlights the need for morally and ethically informed policies.

While the contributors to *Open Borders? Closed Societies?* challenge the exclusionist, national interest-driven approach to the issue of immigration and refugees by upholding the cherished liberal principle of equal moral worth of individuals, George J. Borjas's *International Differences in the Labor Market Performance of Immigrants*—an empirical work using sophisticated statistical analysis to compare earning abilities of immigrants in Australia, Canada, and the United States—unwittingly lends ammunition to the "statists" by implying that if a state wishes to maximize its immigrants' market performance, its immigration policies should be based neither on ideological (e.g., White Australia policy) nor on humanitarian (reunification of families) concerns. According to Borjas's data, the economic performance of immigrants who entered Australia after the repeal of White Australia policy was higher than when that policy was in effect. Similarly, Borjas argues that the 1965 amendment to the U.S. Immigration Act, by "institutionalizing the concept of 'family reunification' . . . led to a fundamental de-emphasis of occupational and skill requirements" (p. 10), which in turn contributed to the decrease in post-1965 immigrants' earnings (p. 97). Since he assumes that people (and by inference states) are "wealth maximizers," it follows that a state should choose immigrants according to its economic needs. He has nothing to say about refugees.

Keith W. Yundt, in *Latin American States and Political Refugees*, modifies the policy implications of Borjas's study by suggesting that unless a state's economy is sufficiently developed, it is not likely to profit from the skills the aliens may bring. He argues that during the interwar years, though the relatively dynamic Brazilian and Venezuelan economies profited from the availability of skilled European refugees, the sluggish economies of Paraguay, Costa Rica, and Columbia failed to gain from such an inflow.

Though the above comparison may suggest that the three books under review inform and illuminate each other, it would be incorrect to infer that either the objectives or the analytical tools of Borjas, Yundt, and the contributors to *Open Borders? Closed Societies?* are similar. The six essays by Frederick Whelan, Joseph Carens, John Scanlan and O. T. Kent, Peter and Renata Singer, Andrew E. Shacknove, and Mark Gibney and Michael Stohl in *Open Borders? Closed Societies?* use history, moral philosophy, and empirical data to question liberal communitarianism and make a sound case for morally responsible and ethically defensible immigrant and refugee policies.

Yundt's study is concerned with neither communitarian nor human rights. His objective is to treat political refugees in Latin America as an issue area and use the international regime framework derived from Keohane and Nye's *Power and Interdependence* (1977) and Krasner's edited *International Regimes* (1983) to organize a dizzying quantity of facts collected from a variety of governmental and nongovernmental sources. He organizes these facts to identify the rules, norms, procedures, and institutions governing political refugees in Latin America. These collectively define the refugees regime in the Latin

American region. The data suggest an ineffective regional regime with weak links to the global refugees regime. This reader found the detailed descriptive data tedious and the conclusions not unexpected. The study's value lies in presenting information on political refugees for each country in Latin America in one book.

Finally, Borjas, an economist by profession, focuses on economic factors and employs simple regression analysis to measure and compare wage growth rates for migrant and indigenous populations in Canada, Australia, and the United States. (I caught one inconsistency, p. 51, equation 16.) Borjas's study should appeal to policy makers interested in selective, desirable skills-based immigrant laws.

Two glaring gaps that mar all three books are the complete disregard of the special needs and problems of women immigrants and refugees and the exclusion of Britain and Western Europe from the discussion of Western nations' responses to immigrants and refugees. This exclusion makes it possible to ignore the question whether or not excolonizers have special obligations to immigrants and refugees from their excolonies. Barring these omissions I found *Open Borders? Closed Societies?* the most thought-provoking and engaging work of the three.

SURESHT RENJEN BALD

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Labor Divided: Austerity and Working Class Politics in Contemporary Italy. By Miriam Golden. Ithaca: Cornell University Press, 1988. 270p. $31.50.

Italian Communism in Transition: The Rise and Fall of the Historic Compromise in Turin. By Stephen Hellman. New York: Oxford University Press, 1988. 274p. $34.95.

For anybody who wishes to understand the behavior of Italian labor and the Left in the crisis-ridden seventies, these two, in many ways complementary, volumes are required reading. The crisis, with elements of economic stagflation, social and labor mobilization, and realignment of the traditional party system, was not unique to Italy; but some of its manifestations were. Two central ones are relevant to the volumes under review: the rapid rise of the Communist party (PCI), advancing the strongest bid for governmental power since the war, and the parallel rise of a politicized, united, and militant labor movement, claiming to replace the political parties as originator of public policies and agent of social change. At the time, decades of Christian Democratic (DC) monopoly of government predicated on the delegitimation of the PCI and the marginality of politically divided labor unions appeared to be coming to an end. To many analysts, impressed by the general discredit of DC-dominated governments and by communist progress, it looked like the PCI and labor were approaching, to use Gramsci's term, hegemony over Italian public life. But from the perspective of the eighties—indeed already from that of the late seventies—those hegemonic threats have proven largely empty. The PCI is now back to its electoral strength of 20 years ago and, having shelved its proposal for a "supraclass" (in Golden's word) historic compromise, waffles about its strategic options. Political divisions hamper a retrenching labor movement. Working from that perspective, both Hellman's and Golden's innovative research goes a very long way in explaining what was wrong with looking at the strategies of the PCI and labor in the seventies as well-oiled hegemonic designs. The PCI, which is the almost-exclusive focus of Hellman's study, failed mainly because of strategic faults in the proposal for a historic compromise, as well as closely connected faults in its obsolescent machinery as a hybrid mass-cadres party. As to labor, the focus of Goldman's study, her entire research is dedicated to demonstrating that the labor movement differed internally, along geographic and vertical lines, in its public policy orientations, and in its relations to the PCI and its strategies for access to government. This impeded a smoother fusion between communist and labor projects. Golden refers to the predicament as one of competitive symbiosis, the terms of which were set from case to case by the unions as much as by the party, often with less than reciprocal satisfaction. In particular, the final support the peak labor confederations gave in the late seventies to a communist entry into government and to its concomitant call for policies of austerity did not reflect communist domination of the unions. It reflected, as Golden documents, unions' final awareness of their inability to replace party and parliamentary politics with direct action, and their decision to revert to traditional demands for a radical change in government. But since, as Hellman argues, the proposal for a historic compromise with austerity proved ineffective, the failure of labor's design for hegemony (if it ever had unity and cohesiveness) merged with that of the PCI.

But, to revert to Hellman, why did the historic compromise come to nothing? In essence, because it originated as a timid defensive strategy to prevent a backlash against the labor and political conquests of the late sixties and early seventies and to build the party's democratic legitimacy. It remained such even after the party's popular support expanded dramatically. It was not so much a popular strategy to reform society—which would have required social mobilization—as an elitist one to "reform" government by opening its doors to the PCI, after which reforms could come. Hence, the compromise emphasized acting through political institutions rather than through society. One can argue with the one-sidedness of Hellman's interpretation: defensive though the strategy may have looked to many on the Left, it looked aggressive and much riskier to other forces. Nonetheless, the strategy did founder (for Hellman's reasons) both inside the party and in the eyes of many supporters. After accounting for the national evolution of the historic compromise in the first part of his study, Hellman focuses in the second part on the intricate repercussions that the compromise had on Turin's Communist party in the context of a rapid ascent to city government. The historic weakness of the party in the city of FIAT, its difficult relations with the unions, its sudden control of city hall (which tested the party's human and organizational resources), and its structures (which still reflected Togliatti's postwar vision of a *partito nuovo)* fulfilled the compromise's local decline. Particularly significant are Hellman's observations on the inability of the *partito nuovo*—a hybrid between the Leninist party of dedicated cadres and the social democratic mass party—to understand the changes of social representation and social demands brought about by the seventies. Organizationally, the failure of the compromise is the failure of the *partito nuovo*. And Hellman offers ample evidence in his analysis of party cadres, their profiles, work habits, and attitudes toward the party and its strategies. All of this is presented with the verve of a sympathetic participant observer who has spent long months studying the party in Turin and has firm opinions about what is right and wrong with the cherished object of his study.

But Hellman's Turin is the city of FIAT, the city with the highest industrial concentration in Italy. Though Hellman investigates local relations between labor and the PCI, his focus is the party. To detect labor behavior fully, one must turn to Golden's volume. Also the product of sympathetic participant observation (the author advocates empathic understanding as a method and speaks of her study as organizational ethnography) this is methodologically the tighter of the two volumes. It makes for more difficult reading, but the rewards are considerable. Golden, too, spends a sizable part of her analysis on the historic compromise, offering an assessment not dissimilar from Hellman's. But it serves as a background to her central interest in labor. If the PCI and the labor confederations converged on a policy of moderation and economic austerity in the late seventies, this should not hide either the relative autonomy of the two (Golden's competitive symbiosis) or the internal diversity of the latter; for Golden's final interest is to understand why in the larger context of labor moderation (a relative one, after all, since it involved a temporary cooling off after already substantial conquests) some labor components (the united metalworkers federation and some, but not all, of its local units) resisted austerity. Golden's sustained analysis of the federation and four contrasting local cases, including Turin, shows that the differences have little to do with socioeconomic, partisan-political, or ideological factors or with rank-and-file demands. These are at best mediated by strictly organizational imperatives, whose logic continues to operate once implanted. To give in one sentence the gist of a tour de force of more than two hundred pages—radicalism emerged most clearly in local unions where especially strong, newfangled, interunion factory councils had already emerged following the wave of militancy of the late sixties. Within such unions there developed an organizational logic tied to the centrality of councils, therefore putting a premium on collective activism, strikes, and political confrontation with management. In other words, those unions could legitimize and reproduce themselves only by reproducing activism and militancy and by projecting it outside the factory. They opposed austerity not because of austerity per se but because it impeded their activism, their right to pressure and bargain, their reason to exist.

The implications of this for the relations between labor and the PCI are rather intriguing. Strong factory councils emerged—in a way by default—where the Communist party's presence in the factory was weak and unable to buttress union presence. This comes close to saying (a similar observation is made by Hellman) that the PCI's proposal for a supraclass historic compromise in a context of austerity could have rallied labor and other forces more forcefully than it did only if the social presence of the party—its ability to hegemonize local constituencies, to coopt labor and other interests—had been more uniformly spread through the country. As it is, this model of social presence and penetration, transcending the obsolescent and stiltedly defensive *partito nuovo* hybrid, only occurs in the region of Emilia-Romagna. There, but not elsewhere, the historic compromise (or a diluted, supraclass, social democratic variant of it) is in some ways a daily reality. In the final analysis, it is the local diversity of Italian political society, the checkered geography of partisan implantations, and in particular the different relations between PCI and organized labor that frustrated the PCI's hegemonic project. It is in turn the strongly rooted nature of many of those partisan implantations—the social pervasiveness of the Italian parties in general—that frustrated labor's earlier aspirations to overtake the political parties.

In conclusion, these are skillfully researched volumes, presenting an intricate panorama of findings, often running against pat and complacent notions about their subject matter. They augur well for the third and fourth generations of students of Italian politics, to which, by my own actuarial calculations, the two authors belong.

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Agrarian Reform Policy in the Dominican Republic: Local Organization and Beneficiary Investment Strategies. By Ana Teresa Guiterrez-San Martin. Lanham, MD: University Press of America, 1988. 290p. $26.50.

The Agrarian Question and the Peasant Movement in Colombia: Struggles of the National Peasant Association 1967–1981. By Leon Zamosc. New York: Cambridge University Press, 1986. 289p. $42.50.

Although the focus of both these books is peasant organization, the two authors' approaches contrast sharply. Guiterrez-San Martin analyzes the success and failure of various agrarian cooperatives in the Dominican Republic with the hope that her conclusions can improve policy making about cooperatives in that country and elsewhere. Zamosc's concern is structural change that will incorporate the entire Colombian peasantry, especially its poorest sectors. This goal was far from achieved by Colombia's national peasant association, and Zamosc's study is highly critical. His approach is historical.

Guiterrez-San Martin's work is a substantial contribution to the study of agrarian cooperatives. Guiterrez-San Martin begins with the acknowledgement that overall, agriculture in the Dominican Republic has stagnated and that the country's agrarian reform (which benefited about 15% of the rural population) has not helped agricultural production. Wisely eschewing a blanket condemnation of agrarian reform on this basis, Guiterrez-San Martin selects three different cooperatives and analyzes the reasons for their performance. Her initial hypotheses are drawn from studies of rural organization by Mitchell Seligson, Norman T. Uphoff, and Milton J. Esman.

Guiterrez-San Martin concludes that group cohesion is the most crucial variable conditioning the economic success of the cooperative. Although other scholars have made this point, Guiterrez-San Martin elaborates on it in an innovative way. In particular, she discusses the experience of one Dominican cooperative that performed poorly despite abundant resources and attributes the failure to the noncohesive members' "escape strategy": they disinvested in their cooperative in order to pursue individually more attractive economic alternatives, which were often urban-based. In her argument that peasants' "love of survival" is greater than their "love of land" the author corrects an analytical imbalance in the literature (p. 221).

One of the greatest strengths of the book is its sensitivity to nuance and to the potential for organizational change. For example, in a particularly interesting section on the evolution of the legal structure of the Dominican cooperatives, the author shows how the reform beneficiaries themselves modified the structure of the enterprises to ameliorate the free-rider and moral hazard problems that plagued the original enterprise model.

Zamosc's study of the National Association of Peasant Users (ANUC) is likely to be the definitive work on this topic. The book is the fruit of more than seven years of research, including both extensive archival research and 148 interviews with government officials, peasant leaders, and rank-and-file members of the ANUC. The author's data on the land invasions launched by the ANUC during the 1970s are particularly rich and should be invaluable to other scholars studying peasant movements in Colombia.

The text of the book amply supports the author's conclusions in his final chapter. Zamosc attributes the failure of the ANUC to transform Colombia's agrarian structure to repression; a modicum of agrarian reform and development programs that coopted the better-off sectors of the peasantry; new occupational alternatives in capitalist enterprises, on marijuana farms, and in Venezuela; the increasing class heterogeneity of the peasantry that complicated strategizing for the ANUC; and the elitist style of ANUC leaders, who were inclined to both traditional clientelist behavior and extreme Left vanguardism.

For the political scientist, the shortcoming of Zamosc's book is that his analysis includes no surprises. Similar points were made almost 20 years ago in an article by Henry A. Landsberger and Cynthia N. Hewitt that was the concluding chapter of the renowned *Agrarian Problems and Peasant Movements in Latin America* (1970) edited by Rodolfo Stabenhagen. More recently, Mitchell Seligson reaffirmed the obstacles to successful peasant organization for the Costa Rican case in his respected study, *Peasants of Costa Rica and The Development of Agrarian Capitalism* (1980). A sociologist, Zamosc admits the lack of a broader theoretical perspective in the introduction to his book.

Nor does Zamosc address a number of questions that are potentially very interesting: Exactly how much upward peasant mobility occurred in Colombia relative to other Andean nations? To the extent that peasant mobility seems to have been greater in Colombia than elsewhere, are there other factors than those mentioned by Zamosc to explain this phenomenon? Has Colombia's unusual political regime mattered? Since political violence continues in Colombia despite the ANUC's demise, what lessons did Colombia's current revolutionary leaders infer from the ANUC's experience?

Both these volumes enhance our knowledge of peasant organization. Both are cogent, accurate treatments of important questions. However, they are also narrowly focused. Perhaps in part because Zamosc's study is so rich in factual detail, his lack of theoretical ambition and comparative context is disappointing. Although Guiterrez-San Martin's work does not provide as much new factual information, her analytical argument is more thoughtfully developed.

CYNTHIA MCCLINTOCK

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Nationalism and Society: Germany 1800–1945. By Michael Hughes. Baltimore: Edward Arnold, 1988. 237p. $14.95 paper.

The Unmasterable Past: History, Holocaust, and German National Identity. By Charles S. Maier. Cambridge: Harvard University Press, 1988. 227p. $22.50.

The "unmasterable past" depicted by Charles S. Maier, the Harvard historian, remains the national burden of the two German states and Austria. For example, in November 1988 it led to the resignation of the Speaker of the West German parliament, and in May 1985 it was responsible for Chancellor Kohl's and President Reagan's humilating experience with the phenomenon Maier calls "Bitburg history"—"the multiple muddying of moral categories and historical agents" (p. 13).

Maier's book was provoked by the *Historikerstreit*, the historians' conflict, a fervant feud among West German academicians that began in the summer of 1986 in the weekly *Die Zeit* and has since preoccupied not only the historians but also political scientists, philosophers, and many others. The central issue is whether the Nazi crimes, especially the Holocaust, were unique—"a legacy of evil in a class by themselves, irreparably burdening any concept of German nationhood" (p. 1)—or whether those crimes were comparable to other nations' atrocities, such as Stalin's terror. The main protagonist on the side of comparability, the Berlin historian Ernst Nolte, maintains that Auschwitz was indeed unthinkably dreadful but "as only one specimen of genocide." Accordingly, Germans can still aspire "to reclaim a national acceptance," as was granted to the Soviet Union in spite of the horrors of the Stalin period (p. 1). Nolte's principal antagonist, the Frankfurt philosopher Jürgen Habermas, charges that the "comparatists" are trivializing the Holocaust, giving academic respectability to the kinds of insinuations reiterated previously in the ultrarightist *Soldatenzeitung* and at SS reunions. While Nolte pretends to be engaged in appropriately scholarly comparative analysis of genocide, his real agenda relates to the growth of a new conservative identity merging "Cold War NATO militarism with traditional German nationalism" (p. 47).

Maier's treatment of these delicate issues is masterful. While admitting to a personal bias favoring Habermas, his analyses of the arguments are painstakingly fair and never superficial. He is convinced that the *Historikerstreit* cannot be reduced to the level of a conservative conspiracy by a "NATO-German Nationalist" clique. But also, Treblinka was indeed unique: however brutal, the Soviets "did not set out to exterminate as an end in itself" (p. 82). Yet Jewish suffering must not be perceived in "exclusive" terms. Maier agrees with the Israeli Jewish theologian who condemned that "new Holocaust religion that refuses to credit other genocides as equally authentic" (p. 166). However successful Western Europe may have been since 1945, it has also known "a history of forgetting; forgetting fascism, Nazism, and collaboration" (p. 161). Not just the Germans resent being reminded of the Nazi era. The troubled French confrontation with the memory of Vichy and the Austrian catastrophe around Waldheim are typical examples of a widespread avoidance of a seemingly unmasterable past.

Another facet of the *Historikerstreit* developed around the issue of Germany's supposed *Sonderweg* ("separate road")—the assumption that a specifically German set of psychological and institutional factors were responsible for the Hitlerian period. Habermas fears that the culture that made Nazism possible in 1933–45 might still "transmit" Nazism today. The Germans must at all times be aware of their legacy: "The Holocaust must be as fundamental to German identity as it was to Israel's" (pp. 58–59). These kinds of assertions are denied by Nolte and his allies who assume that more general, extra-German, political, geopolitical, and socioeconomic factors explain the Nazi period.

Michael Hughes, a British historian, gives an account of the development of German nationalism in the period 1800–1945, providing valuable insights into this aspect of the historians' conflict. (Hughes's book also contains a superb chapter in which he attempts to define nationalism.) He insists that until the last quarter of the nineteenth century nationalism was "a minority-movement, deeply divided and with only a marginal impact . . . on political life" even in Germany (p. 2). It was only after 1871—and then particularly in Germany—that nationalism became a "dangerous ingredient in a very unpleasant brew" (p. 7). An older, more liberal strand of German nationalism was replaced by a *völkisch* ("folkish") type based on resentment and frustration, which was duly exploited by the German conservatives for manipulation of the masses. To be *völkisch* meant to romanticize the past, to reject modernity, and to hate foreigners, especially those "ultimate foreigners," the Jews. Paradoxically, this kind of nationalism reached its fullest flowering during the Weimar period, and Nazism fitted right in. Hughes does assume a sort of German *Sonderweg* in the years 1875–1945. Today, German nationalism has gone back to the type prevailing before the 1860s, with the population no longer very interested.

Maier does not think much of the *Sonderweg*: it is "asking too much from history." Historical paradigms cannot be that rigid. After all, "after Vietnam the once-reassuring idea of an American *Sonderweg* has also lost credibility" (p. 120). Maier does not object to the "legitimate" comparative analysis of genocide systems since such analysis "will enable the historian better to understand the specificity of each such murderous episode" and to find, perhaps, "common causal mechanisms," a sociology of genocide. What is "problematic" is the use of the comparative method "to remove the emotional or moral burden of . . . uniqueness" (p. 70). We cannot build the future by forgetting the past, however unmasterable. "The answer to obsession is not forgetting, but overcoming" (p. 168).

Maier's erudition, mastery of the German literature, and sense of fairness have created balance where balance seemed hardly possible. Hughes's essay on nationalism fits in most appropriately, providing pointed yet "cool" insights on issues so fervently debated in the *Historikerstreit.*

HENRY L. MASON

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The Thinking Reed: Intellectuals and the Soviet State from 1917 to the Present. By Boris Kagarlitsky. London: Verso, 1988. 374p. $29.95.

The relationship between intellectuals and the state in both Czarist Russia and the Soviet Union has been complicated and troubled. Boris Kagarlitsky explores this relationship, chronicling the long-standing cleavage between a hegemonic bureaucratic elite and an intelligentsia that has constituted its most viable source of opposition. His account is exhaustive in covering not only the diversity of artistic, scientific, and academic elements comprising the Russian intelligentsia but also the developmental phases through which the Soviet polity has evolved since 1917.

Central to the analysis is Kagarlitsky's notion of a "statocracy" ruling the USSR. The statocracy, or privileged bureaucratic elite (referred to by some as the *nomenklatura*), began consolidating its power in the earliest days of Soviet power. Its leading members may have championed a commitment to revolutionary change and a vibrant new socialist society, but they quickly parted ways from those in the intelligentsia who were once their allies. From the beginning they were motivated by an interest to expand and maintain their influence, grounded in both economic and political power. Kagarlitsky argues that they never represented the working class. But then, he sees at best a tenuous link between the intelligentsia and the working class.

The author identifies a "logic" to the statocracy's policy toward intellectuals, artistic or scientific, namely, to bring their creative thinking down to the level of common bureaucratic thinking and therefore assure state control (p. 121). Much of the volume is a painful account of how the bureaucratic elite has guided the cultural-political life of the country while assuring that control. Yet as Kagarlitsky convincingly demonstrates, the intelligentsia is not without its resources. It has possessed a self-consciousness, a certain "social maturity," and a position that have permitted it to survive, and occasionally flourish, in the course of constant persecution. Resilient in the face of challenges of the long reign of Stalin, the intelligentsia recovered from the disappointments of the Khrushchev period and survived the backsliding of the Brezhnev years. Indeed, Kagarlitsky identifies a number of important new cultural and philosophical currents that emerged during the stagnant Brezhnev era.

The author is uniquely qualified to assess the complex existence and role of Soviet intellectuals. An active member of the USSR's contemporary democratic renewal movement, Kagarlitsky is one of the founders of the Federation of Socialist Clubs. Imprisoned during the final year of the Brezhnev regime, he was released 13 months later and has subsequently assumed a high profile in the Russian social democratic movement. As is evident in this work, his worldview has been especially influenced by Roy Medvedev. It is not surprising that his interpretation of the country's intellectual history is subjective and at times highly personal. The author nicely captures the excitement of the first decade of Soviet power, when the intelligentsia assumed an important role in the formation of the new society. Likewise, he effectively conveys the exuberance of the post-Stalin thaw and even the potential of the late Brezhnev period. There are contentious specific arguments (e.g., that the political theories of Sakharov and Amalrik "are hardly to be taken seriously" [p. 214]). However, there are fresh assessments of past problems (e.g., the legacy of Asiatic despotism) and contemporary dilemmas confronting Soviet society that merit readers' attention.

This volume presents a challenging read, both in its breadth and in its presentation. It is comprehensive, as the author deals with the cultural-political history of a diverse country. While its focus is on Russian intellectual life, there is some attention to that of other Slavic and Baltic peoples. Kagarlitsky is up to the task. He is intimately familiar with the thought and writings of contemporary and past Soviet and Russian intellectuals while also well versed in relevant Western scholarship. If the author is to be criticized, it is for sometimes failing to tie the individual schools of thought and groups of intellectuals into a more coherent whole. The direction of Kagarlitsky's discussion is not always evident as the reader wades through 65 years of Soviet intellectual history. Perhaps his task is an overly ambitious one, but the reader will profit from a perusal of this work.

The book was completed at the time of the author's imprisonment in 1982, but the analysis does anticipate some of the changes that emerged in the wake of Brezhnev's death. Many readers would wish that when published in the West, this book had been updated to include post-Brezhnev developments. The editors only partly remedy this by adding a second part to the volume with articles written by Kagarlitsky during the era of *glasnost.*

Kagarlitsy contends that throughout the USSR's cultural-political history the role of intellectuals has been critical, yet not sufficient to assure democratic societal change. For such change to occur, the proletariat must gain the will and leadership to join with those intellectuals in differentiating their interests from those of the bureaucratic elite. Kagarlitsky argues in part 2 of the volume that this is in fact now beginning to occur, as the intelligentsia is reinvigorated. Ongoing developments in Gorbachev's USSR will be most interesting to follow as we assess the veracity of Kagarlitsky's contentions.

JOHN P. WILLERTON, JR.

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Trade Unions and Socialist Politics. By John Kelly. London: Verso, 1988. 333p. $50.00 cloth, $14.95 paper.

The New Unionism: Employee Involvement in the Changing Corporation. By Charles C. Heckscher. New York: Basic Books, 1988. 302p. $22.95.

Has the "forward march" of labor been stopped in the advanced industrial states? These two books take opposite sides in what has become a fierce debate. John Kelly argues that the British labor movement remains strong and well positioned for realizing a transition to socialism. Charles Heckscher contends that U.S. labor's deep crisis requires a basic reorientation of its ends and organization. Each, however, asserts that his analysis of trade union strategy applies to labor in other advanced nations. Unfortunately, Kelly's argument about labor's strength fails to take adequate account of the fundamental social changes and union developments highlighted in Heckscher's study.

Kelly uses classical and revisionist Marxist analyses of trade union strategy to discern what conditions generate a radical class consciousness and promote a socialist transformation. Contending that the political and economic situation, rather than internal organizational factors or workers' economic interests, constitutes the main cause of radical working class behavior, his survey of the classical Marxists' analyses of union strategy defends a theory of mass politicization, especially Luxemburg's theory of mass strikes. In contrast, Heckscher adopts a pluralist framework, arguing that the Wagner Act embodied the notion of countervailing power between labor and capital and that the recent increase in the diversity of U.S. society demands a more participatory and multilateral type of union organization.

These analytic perspectives inform each author's view of the current predicament of labor movements. For Kelly, the cyclical character of working class radicalization points to continuity and resilience in trade union institutions, in spite of short-term setbacks. Thus, the recent decline in membership, strike frequency, and bargaining power among British unions reflects a trend similar to that of the 1930s, not a historical turning point. To the contrary, Heckscher highlights the changing nature of the community of workers and the inadequate response of the U.S. labor movement to these changes. As a result, he sees labor in a deep crisis, much like the period of the 1920s and 1930s, when it made the transition from craft to industrial unionism.

Moreover, Kelly's revival of Luxemburg's mass strike theory and Heckscher's pluralist assumptions suggest the forces shaping union strategies. For Kelly, strike waves not only are cyclical but, when provoked by state intervention, polarize labor and capital and make possible a socialist transition. He cites the 1915–22 and 1968–74 British strike waves (but rejects that of 1977–79) as evidence of the impact of mass strikes and the state's response to them on political consciousness. As a result, Kelly argues that the materialism of workers and the divisive sectionalism of unions are not insurmountable obstacles to political mobilization, that union leaders are not inherently more conservative than their memberships (oddly ignoring whether leaders are accountable and responsive to their members), that industrial democracy subordinates labor to the overwhelming pressure of markets, and that the state can be made to realize workers' interests (though a Swedish strategy would be inappropriate for Britain).

Heckscher locates the forces for change in major societal transformations. First, unions now confront a dramatically changed economic environment; a move from mass production toward speciality production, from rising to falling real wages, from a need for predictability to individual responsibility and flexibility, and from strikes to alternative measures. Second, the emergence of a more professional work force of semiautonomous individuals who use their intelligence in the service of the firm undermines the formal organization of industrial unions and promotes less bureaucratic, more participatory associational activity. Third, the growth of issue-based associations like environmental, consumer, and civil rights groups that promote their interests through publicity and enforce private rights through court action now compete with unions, winning employees' new rights by law rather than by collective bargaining. Most fundamentally, the recent explosion of rights redefines the terrain on which unions organize, weakening their organizational power.

Each analysis of change entails a different union strategy. Having argued for the continuity of labor in institutional terms, Kelly contends that socialism in Britain depends on a renewed mobilization of working class industrial action in conjunction with the support of various progressive movements. Traditional unionism headed by an inspired political party during a period of mass unrest can lead to socialism. On the other hand, citing cases like the Sarnia Shell chemical plant and the Detroit Pontiac Fiero plant, Heckscher calls for a new type of labor strategy. This new "associational unionism" is participatory and antibureaucratic. Its main features include a commitment to general principles, increased participation, multiple forms of representation, training, diversity of tactics, and extended alliances with other interest-based groups. Relying on employee rights, associational unionism practices multilateral negotiation, involving a complex network of those affected, and affords workers better representation than legal, administrative, or managerial control of industrial relations.

While associational unionism is a speculative notion derived from a selective use of instances of participatory industrial relations, Heckscher convincingly highlights fundamental changes in the bases and character of labor movements. Focusing on how workers define their relationships, social meanings, and aspirations, he identifies important new directions in how workers relate to industry. Yet Heckscher fails to go beyond a pluralist conception of society or to examine questions about domination, exploitation, and inequality.

While Kelly's Marxist analysis assumes a concern for these issues, his picture of labor is anachronistic, failing to capture the changed attitudes among British and other workers about class relations. Kelly's use of organizational indicators like membership and votes to document the strength and resilience of labor ignores changed attitudes, democratic practices, and internal power relations. If he had followed Luxemburg more faithfully by taking seriously the mood and motivation of workers, he would have assessed the dilemma facing organized labor in Britain and elsewhere more accurately—a loss of class community and aspiration.

JOEL D. WOLFE

University of Cincinnati

Controlling Corruption. By Robert Klitgaard. Berkeley: University of California Press, 1988. 210p. $19.95.

Questions of morality, Robert Klitgaard reminds us, are unavoidable for analysts of politics. Chief among these issues, particularly in the developing world, is corruption. We cannot ignore corruption, he argues, nor should we rationalize it away. And once we have come to grips with the problem, we must try to do something about it.

Klitgaard's book analyzes strategies and tactics for fighting corruption with an emphasis on developing nations. Several case studies form the heart of the book. A menu of anticorruption policies is developed in the successful cleanup of the main tax collection agency of the Phillipines. Hong Kong's successful Independent Commission Against Corruption and Singapore's anticorruption campaigns are placed into that framework. A particularly interesting chapter takes up the U.S. Army's efforts to deal with bid rigging by local suppliers in Korea. Reforms must be implemented with great political sensitivity, however, as is made clear in the discussion on a failed crusade against corrupt grain distribution in a developing nation. The author extracts policy lessons from the case studies and admonishes scholars to confront the moral issues of development head on.

Klitgaard uses a principal-agent-client framework to define corruption and to identify remedies for it. Incentives and sanctions are conceptualized mainly in material terms. He has a laudable no-nonsense attitude toward some of the dilemmas of studying corruption. Rather than shying away from making value judgments in developing settings, he makes the reasonable argument that most corruption is harmful most of the time and then moves on to the question of what to do about it. For example, agents can be selected for qualities of honesty and ability, incentive structures can be changed, more information on agents' activities can be gathered, the principal-agent-client relationship can be restructured, and attitudes about corruption can be changed (pp. 94–95). Klitgaard recognizes that anticorruption efforts have their costs, too. The "optimal level of corruption" will thus be greater than zero because at some point the costs of further reductions in corruption will exceed the marginal benefit.

Taken on its own terms, this is a sound analysis. No miracles of reform are promised, but recommendations are well grounded in the principal-agent-client framework and reflect Klitgaard's impressive degree of experience in development planning and administration. But those who study corruption theoretically may find some things missing. Klitgaard's approach offers an opportunity to link anticorruption policies to more general concepts of culture, ethics, and democracy; yet this is seldom done. Indeed, at several junctures the author pointedly dismisses—unfairly, in my judgment—the academic literature on corruption as hopelessly tangled in its own equivocations or as altogether irrelevant. Scholars who point out the complexities involved in defining and assessing corruption are labeled "apologists for corruption" (p. 35). The book does not begin to cover the academic literature broadly enough to support such a verdict.

Moreover, it could have been enriched by some academic insights. Klitgaard uses the principal-agent-client framework not only to propose reforms but also to *define* corruption. This attempt, resting heavily on generalizations about the principal's "interests," is unsuccessful. Similarly, he recommends changing attitudes toward corruption with little reference to the cultural complexities this might entail. Finally, the analysis raises major questions of democracy and justice but does not examine them in much detail. While the author acknowledges that some anticorruption tactics in Hong Kong and Singapore would elsewhere be seen as threats to liberty, there is little general discussion of how fighting corruption fits in with broader social goals and values. In each of these areas, the academic literature could have imparted considerable richness to the analysis.

Still, this is a most worthwhile book, both for its insights into anticorruption policies and for the academic debate about more general questions that it will certainly create. It is good to know—and to know *how*—something constructive can be done about corruption, even in the Philippines of Ferdinand Marcos.

MICHAEL JOHNSTON

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Prelude to Solidarity: Poland and the Politics of the Gierek Regime. By Keith John Lepak. New York: Columbia University Press, 1988. 217p. $35.00.

Poland Challenges a Divided World. By John Rensenbrink. Baton Rouge: Louisiana State University Press, 1988. 246p. $19.95.

The political and economic crisis that wracked Poland at the beginning of the 1980s continues to attract the efforts of Western analysts to understand and explain both its origins and its long-term impact. The two books under review contribute to that growing literature and deal, respectively, with the background to the Solidarity crisis and its implications for both East and West.

In *Prelude to Solidarity* Keith John Lepak presents the most comprehensive political assessment in English to date of political and economic developments in Poland during the 1970s. He argues that in some respects, the problems of the 1970s—in fact, of communist Poland more generally—paralleled those of interwar Poland, for they have been rooted in the inadequacies of the political system, Poland's weak international position, the problems of societal modernization, and failures of leadership. In none of these areas has communist Poland been any more successful than its predecessor in resolving key problems.

Within this general framework Lepak examines in some detail the issues involved in the rise and fall of the Gierek regime and the exacerbation of the economic and political problems that led to the Solidarity challenge of the early 1980s. Despite the promise of the early Gierek years, Lepak maintains, by the end of the decade the compounding of bad decisions and poor management, plus the policy immobilism that gripped the political elites created an environment conducive to the birth of Solidarity and its challenge to continued communist dominance.

While Lepak is concerned with explaining the historical roots of the Solidarity crisis, John Rensenbrink attempts to establish the case that the resolution of Poland's political divisions might provide a model for a resolution of the division of Europe. The two basic arguments of his *Poland Challenges a Divided World* are first, that most U.S. analysts of Polish affairs do not really understand the nature of Polish society and culture and thus employ incorrect categories in their attempts to explain developments in that country and second, that successful resolution of the conflict between Solidarity and the Party in Poland could present a model for the broader resolution of conflict between East and West.

Actually, virtually all of the points made by Rensenbrink concerning the nature of Polish society and the deep-seated drives represented by Solidarity's challenge to continued Communist party control can be found in other studies of contemporary Polish politics. Thus, his book adds little to our understanding of this aspect of the Polish political system. Though interesting, his discussion of the possible ways in which a successful resolution of Polish political differences might provide a microcosm of the resolution of the global conflict between the communist East and the capitalist West is not clearly developed. In many respects this book, which suffers at times from a vague and rambling style, is more a personal statement by the author than it is an academic analysis—although it does include important analytic insights.

Of the two books Lepak's is the more carefully written and effectively argued. It is highly recommended to readers concerned with the "story" of Polish political developments in the 1970s and also to those concerned with a broader interpretation of the meaning of that story. Moreover, the key points that the author makes about the carryover into postwar communist Poland of many of the problems that plagued interwar Poland apply as well to the late 1980s as they did to the late 1970s.

ROGER E. KANET

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Of Rule and Revenue. By Margaret Levi. Berkeley: University of California, 1988. 253p. $35.00.

Taxation by Political Inertia: Financing the Growth of Government in Britain. By Richard Rose and Terence Karran. London: Allen & Unwin, 1987. 264p. $34.95 cloth, $16.95 paper.

Taxation is probably the most significant political phenomenon that political scientists have left relatively unexplored. There are extensive literatures on the economics and law of taxation, but few efforts have been made to understand the role of taxation in state development and behavior or even to begin the formulation of political theories of taxation. Happily, this vacuum has been partially filled by these two outstanding books. The approaches taken and conclusions drawn by Levi on the one hand and Rose and Karran on the other are quite different, yet in many ways the implications of their analyses are complementary. Both efforts should help fuel further discussion in an area that deserves much greater attention.

Levi's central thesis is that rulers are predatory, a tendency manifested in a constant attempt to maximize revenue. The ability to do so, however, is limited by three kinds of constraints: bargaining power relative to other actors in the community, transaction costs, and the discount rate (which reflects the level of concern for the future). Variations in these constraints account for variations in revenue policies, and a change in these constraints in any given case should produce a policy change. This position is built on the assumptions and theoretical approach of public choice theory, but Levi goes beyond methodological individualism. Her concern is to blend individualism and structuralism, recognizing that institutions shape behavior but at the same time insisting that what is being shaped is, after all, individual behavior and that consequently the individual must be included in a theory of the state. To amplify the potential of the predatory model of rule four specific cases are explored: republican Rome, medieval France and England, eighteenth-century Britain, and twentieth-century Australia. The cases are not presented as proof of the thesis but rather as a means of illustrating its utility. The most problematic aspect of the argument is the usage of *ruler* as a unitary category, but by accepting this as a constant, considerable explanatory power is focused on the three key variables Levi identifies as constraints on the ruler.

Rose and Karran argue that taxation is first and foremost a political problem and that tax policy must be understood as a politician's solution to that problem. An elected official needs to spend money on programs popular with constituents but fears the consequences of being associated with raising the revenues necessary to pay for those programs. This political dilemma can normally be solved in only one way: taxation through political inertia. As the economy grows, revenues increase unless tax rates are cut; that is, if tax rates remain constant, revenues increase. This gives politicians room to maneuver, allowing them to simultaneously oppose tax increases (i.e., rate increases) and enjoy increased revenues to spend on popular programs. The authors spell out the circumstances contributing to the logic of taxation through political inertia and pull them together in a concise theory. The theory is illustrated through a historical examination of taxation in Britain, but the authors believe that it has wider applicability as well. Those following U.S. fiscal policy in the 1980s will have some trouble with the theoretical linchpin of the position—the "great identity" of revenue and expenditure, which keeps them inexorably linked—but the future of the U.S. experiment with regular, peacetime deficit finance is still unknown.

On one level Rose and Karran's view of the state's pursuing a tax policy of inertia seems rather passive compared to Levi's dynamic image of the state as predatory. Despite the contrasting language, however, they can be seen as highlighting different sides of the same whole. In Levi's terms, Rose and Karran are describing the particular strategy which best allows rulers in stable, prosperous democracies to maximize revenue. The constraints confronted by elected officials in these circumstances dictates relying on inertia to fund government growth, a strategy that is not as obviously predatory as may be the case with an absolute ruler but has nevertheless been effective in expanding the revenues available to government.

Combining the broad approach of Levi and the more specific insights of Rose and Karran helps delineate the questions that remain to be addressed. Levi's approach is particularly useful for explaining change across varying political structures, while Rose and Karran provide a more detailed insight into the predominant dynamic within a particular structure, advanced industrial democracies. What remains is to pin down the sources of change within this contemporary structure. Rose and Karran's incrementalism does not produce a theory explaining variation within advanced industrial democracies, and Levi looks at change across structures. To explain variation within the modern state structure, both across states and within one state across time, an understanding of the variables characteristic of advanced industrial democracies that constrain political actors is required. Fortunately for political scientists, these two fine efforts provide a solid base for further work in this important area.

JAMES L. CURTIS

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The Political Language of Islam. By Bernard Lewis. Chicago: University of Chicago Press, 1988. 168p. $14.95.

The Islamic Question in Middle East Politics. By Allan R. Taylor. Boulder: Westview, 1988. 150p. $29.95 cloth, $14.95 paper.

The Politics of Islamic Revivalism: Diversity and Unity. Edited by Shireen T. Hunter. Bloomington: Indiana University Press, 1988. 303p. $35.00 cloth, $12.95 paper.

Just two decades ago the literature on politics of Islam was sparse and scattered. What was available was all too often a compendium of mystery and misinformation. In the past decade, however, partially due to political impact of the Iranian revolution and the wave of Islamic revivalism that had engulfed different parts of the Muslim world since the 1970s, a plethora of writings have appeared on multifarious dimensions of religion and politics in Islam. Various methodological approaches have been developed to study the hermeneutics, historical origins, and social development of Islam. Some scholars have used the method of phenomenology of religion in an attempt to understand the Islamic tradition in its own terms. Others have utilized the method of textual criticism to study the classical texts and scriptures through empirical reasoning. Yet others primarily use a sociological method to study Islam not as a phenomenon unto itself but rather as a social phenomenon reacting to broader socioeconomic forces that mold the different interpretations of the faith facing us in each epoch.

The common underlying similarities of the three books under review is that they all adopt the method of sociology of religion to elucidate the relevance of the past to the present and explain traditionalization of modernity in the age of ideology.

Bernard Lewis's past research on the history of Islam long before the study of the subject matter became popular laid the foundations for much later scholarship. It is not surprising that his current contribution, which began as a series of lectures delivered at the University of Chicago in 1986, provides scholars of the Middle East and the students of religion and politics with a penetrating study of Islamic hermeneutics.

Islam as the ultimate source of identity and cultural loyalty serves as the most potent instrument of political socialization in the lives of the faithful, encompassing the private as well as the social and political realms. As a total faith, it is the most elementary and pervasive common denominator in social perception and in cultural identity. These attributes are deeply embedded and reinforced by the structure of Islamic hermeneutics. The book is an attempt to put the development of Islamic political language in its historical and social context. By doing so, Lewis takes us to the cultural context of political discourse in Islam, a hitherto underinvestigated subject.

The sanctity of traditions among Muslims has rendered the past a powerful source of inspiration. Traditions in the Middle East are woven from that resilient fiber that would not fade away with passing of time—rather, time seems only to reveal its true colors and tenacity. Much of common rituals pervading in Islamic societies are designed to pass the sacred traditions from one generation to another. The new generation can reinterpret or reformulate their meaning to accommodate political and social expediencies of the day but cannot question their sanctity. As Lewis notes, "Few if any civilizations in the past have attached as much importance to history as did Islam in its education, its awareness of itself, in the common language of every day talk" (p. 9). The significance of studying Islamic hermeneutics lies in the fact that "much that in other faith is expressed through art and music is, in Islam, expressed through the word, giving to verbal communication a unique importance" (p. 10). Having laid out the significance of his subject matter, Lewis traces the roots of Islamic political language in the Koranic exegesis, traditions of the prophet, and the practice of early Muslims as well as influences of Persian and

Turkish statecraft and Greek treatises on political philosophy. Where appropriate, the author draws on examples of other religious traditions to put Islamic experience in comparative perspective.

The book is riddled with insightful theoretical observations drawn from the study of such topics as the body politics, the polity, the ruler, and the ruled. Lewis's illumination of such terms as *kingdom* and its negative connotation in Islamic history, for example (pp. 53–55), sheds light on Khomeini's successful attempt to revive this sentiment and mobilize Iranian Muslims against the shah by branding him as *taghut*, the illegitimate Muslim ruler. However, Lewis's assertion that Islamic government until recently cannot be regarded as either theocratic or despotic (pp. 29–31) must be taken with some measure of skepticism. Not only is there a large volume of literature that attests to the contrary, but the primordial rule of the prophet Mohammad by many accounts stands as an example of theocracy. Mohammad was the religious as well as the political ruler of his community. While he introduced the concept of *ijma* ("consensus") that allowed for some measure of consultation with the community, as a divine ruler he was the ultimate judge and the arbiter on matters of law and politics. The Islamic governments of today are inspired by his paradigmatic example.

While Islamic political thought is not innately undemocratic, the propensity of Islam as a total faith renders it susceptible to theocratic absolutism. As Lewis himself notes, "To a legitimate ruler, the Muslim subject owes complete and immediate obedience" (p. 94). Since legitimacy in this case is not institutionally based but divinely ordained, the religious ruler as the ultimate embodiment of God's will cannot be subject to, or checked by, profanely conceived law, such as a constitution.

Taylor's *Islamic Question in Middle East Politics* is an attempt to analyze the roots of "neofundamentalism." Although many of the sources used in this study are secondary and some of the questions raised in this volume have been discussed by others, the book is a useful overview of literature on the roots of Islamic revivalism.

Adopting a historical approach, the author delineates the reasons behind the failure of secular nationalism and liberalism. The historical roots of Islamic revivalism are traced back to the decline of the Ottoman Empire and the partition of Iran in 1907 by Great Britain and the czarist Russia. The ascendence of Western domination in the Middle East throughout the nineteenth and twentieth-centuries was the key catalyst behind Islamic revival in the region. The Islamic empire for all practical purposes ended as a viable cohesive force at the end of the tenth century. But Islam continued to remain a "formidable cultural tradition periodically reaffirmed by movements of revival and reconstruction" (p. 1).

Taylor asserts that the neofundamentalists have severed Islam from its early humanitarian message and that they remain for the most part ignorant of the true history of Islam and "distort aspects of tradition they seek to represent" (p. 13). Having made this assertion, the author attempts to lay out the humanist theme in Islamic worldview. Taylor briefly excavates the constituent elements of Islamic synthesis during its golden age and traces its evolution to modern times. The author concludes, "What is most tragic about this is that humanism, eclecticism and profound understanding of nature and the existential interrelatedness of things, which are found in classical Islamic phenomenology, would be particularly helpful to Muslims as they attempt to cope with many contemporary problems" (p. 25).

The neofundamentalist interpretation of Islam, by imposing its totalitarian image of Islam on the faithful, prevents the revival of this humanist and spiritual tradition. Such interpretation also prevents "the synthesis of Western and Islamic traditions, which formed the basis of Islam's greatest earlier achievements" (p. 28). Taylor's point is well taken, since some of the major contributions to Islamic political thought were made by such philosophers as Ibn Sina, Al Farabi and Mulla Sadra who all came under the influence of Western rationalism and Greek philosophy.

To provide some clues for the declining fortune of nationalism in the Middle East, the author discusses the impact of the major events following World War II, culminating in the temporary prominence of pan-Arab nationalism and Ba'th socialism, the two cornerstones of nationalism in the Arab world. The failure of the Syrian-Egyptian merger of 1958, the clash of Egyptians and Saudi forces in Yemen in 1962, and finally the defeat of Arab armies at the hands of Israel in the June 1967 war were all decisive blows to the ideology of secular nationalism. As the antinationalist leaders failed to deliver on their promises of autonomy, political participation, and economic equality, they began to lose credibility in the eyes of the masses. The young generation and a portion of alienated intelligentsia began to search for alternative ideologies.

Deeply rooted in the domestic cultural soil, a reinterpreted, politicized and revitalized Islam emerged as the banner of cultural authenticity and national identity, rallying the popular masses. As the author accurately concludes, "The problem with nationalism in the Middle East may not be the institution itself but the superficial way it was formulated from only partially understood Western models" (p. 114). In counterposing itself to Islam, the common cultural denominator of popular masses and their true banner of cultural authenticity, nationalism was doomed to failure. In other words, the nationalists' dilemma was that in the eyes of the laity, they could not adopt Western values and champion themselves as the true defenders of national identity.

The author then presents a profile of neofundamentalist groups and their ideologies in Egypt, Turkey, and Iran. Through a case study of Islamic revolution in Iran, Taylor probes the theory and practice of the Islamic republic. Having discussed the intricacies of Khomeni's politics and his rise to power, the author briefly delineates the main tenets of Khomeni's worldview and political conduct.

While Taylor's observation that despite their vehement rejection of Western culture and value, many neofundamentalists have borrowed intellectually from the West (p. 121) is generally accurate, his conclusion that "the open approach of the Islamic modernists is the only viable option" (p. 124) overlooks an important dilemma. To be sure, in the epoch of increasing interdependence, where isolationism and nationalism seem to have become economically obsolete, this seems not only a reasonable hope, but even a desired one. History, however, has repeatedly betrayed both our logic and our wish. The common faith that the modernists share with the nationalists is that they cannot borrow Western ideas in an attempt to represent cultural nationalism to the laity. Moreover, the success or failure of the modernists to a great extent is contingent on Western nations' policies toward the Muslim world. The emergence of a new understanding and a new dialogue based on the realization that the development of state capitalism and Western institutions does not necessarily lead to a supremacy of Western culture in the Middle East may actually help the modernist cause and strengthen their position.

Finally, Taylor's counterposing of humanist Islam to the politicized Islam of the neofundamentalists (pp. 123–25) overlooks an important fact. No matter how reprehensible one may find it, the fact remains that the present political extremism pervading among the neofundamentalists is a direct by-product of abuse of power, deep inequalities, and political repression promulgated by a large sector of Middle Eastern secular elites. Insofar as secular opposition parties are not allowed to function freely and the mosque continues to remain the only refuge from the repression of a lawless state, neofundamentalism will persist as a political organization for propaganda and mass mobilization. Hence, when the legitimacy of the state as the organ that has a monopoly over the use of force erodes and it has to resort to excessive violence to rule, it can only induce extremism among its opponents. In the final analysis, people change the world not because they have a perfect vision of the descending utopia but because they can no longer stand the old order. When they do this, they embrace the leadership that emerges from the spontaneous mass movement. Due to the crisis of secular values in the Muslim world, history has temporarily pushed the neofundamentalists to the forefront to assume the position of leadership. The humanist tradition of Islam has a better chance to prevail under more human circumstances. Until that day, it has to bow before the vociferous expression of frustration, anger, and violence represented by the neofundamentalists.

The third book under review is the result of a study group's deliberations organized by the Washington-based Middle East Program of the Center for Strategic and International Studies in 1985. Although the quality of contributions is uneven in both breadth of analysis and theoretical rigor, overall this is a useful book dealing with the phenomenon of Islamic revivalism on a global level. The essays in this collection deal with Islamic revivalism in the Arab East, namely Egypt, Syria, Lebanon, Iraq, the

West Bank, and the Gaza Strip. Other essays cover the gulf region (Saudi Arabia, Kuwait, and Bahrain), Africa (Morocco, Tunisia, Algeria, Sudan, and West Africa), and Asia (Pakistan, Malaysia, and Indonesia).

Hunter's introduction expresses some skepticism in regard to crisis theory as the predominant causal factor responsible for the revivalist movement. She argues that it was not only the crisis but equally a feeling of increased self-confidence spurred by the initial victories of Egypt and Syria in the Arab-Israeli war of 1973 and the increasing income from petrodollars during the decade of 1970 that induced the revival. DeKmejian (chap. 1) argues that crises, including "identity conflict, legitimacy crisis, political conflict, class conflict, and culture crisis, as well as military impotence" (p. 7) and disillusionment with Western ideologies and development schemes did indeed play a crucial role in the return to Islam.

In chapter 2, Sonbol finds the root of the appeal of Islamic groups in Egypt in the expansion of modern education to the social sectors that have most rigorously maintained their Islamic roots; heightening political consciousness and expectations; and the "villagization of cities" by the deeply religious and not easily intimidated peasant migrants, where a combination of overcrowdedness, lack of opportunities, social inequality, and mounting frustration renders them primary targets for recruitment to the different revivalist groups (pp. 26–29). Whereas in much of the Middle East the revivalist movement constitutes the disaffection of migrant peasants and their leaders against the pro-Western urban elites, in Syria, the Islamic opposition "expresses the reaction of the urban establishment and its mass following in the traditional city against a rural-based regime that has damaged its economic interests" (p. 39).

Robin Wright's investigation into Sunni and Shi'i Islamic revivalist movement in Lebanon (chap. 4) sheds light on the present state of political and religious sectarianism that has shaken that troubled land in the past decade. Chibli Mallat (chap. 5) lays out the foundations of the Iraqi revivalist movement against the communism and the "Sunnization" of the decision-making process personified in the regime of Saddam Husain. The pivotal role of the charismatic leader Mohammad Baqer as-Sadr and the dilemma of the Shi'i majority in a state ruled by the Sunni minority are analyzed (ch. 5). John Esposito's analysis of the movement in Sudan (chap. 12) is penetrating and riddled with insightful theoretical observations. By putting Numari's Islamicization program in the context of modern Sudanese politics, he appraises it as an effective instrument of legitimacy and mass mobilization that has enabled Numari to rule more effectively. Hunter's analysis of Iran's Islamic republic (chap. 16) postulates several significant theoretical points. Against the popular belief, the author concludes that Khomeini's regime actually has accommodated nationalism. She draws out the constituent elements in Khomeini's worldview and concludes that there is a pragmatic element within the Iranian ruling circle that would not allow a total rupture of relations with the West to occur.

This is a comprehensive collection of essays with many new insights and observations on the current state of the global Islamic revival. While some of the essays lack in theoretical depth, and many recurrent themes with some redundancy could have been avoided, overall this collection is notable for the wealth of information it provides. It would enlighten and inform both the general reader and the experts.

MANOCHEHR DORRAJ

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Bureaucratic Authoritarianism: Argentina, 1966–1973, in Comparative Perspective. By Guillermo O'Donnell. Translated by James McGuire. Berkeley: University of California Press, 1988. 338p. $45.00.

Why should a newly published work by an author known for the currency of the topics upon which he writes (see, e.g., his *Transitions from Authoritarian Rule* [1986]) focus on the 1966–73 period of bureaucratic authoritarianism in Argentina instead of the more recent and more intense 1976–83 period? First, he explains, simply because his research, largely completed by the mid-1970s, for various reasons could not be published until now. But also, he urges, the earlier period will reveal the tensions and contradictions inherent in the Argentine bureaucratic authoritarianism that were more fully manifested in the repression and political turmoil of the later period.

Moreover, the book is more than an historical narrative. It refines (or modifies) O'Donnell's theoretical account of bureaucratic authoritarianism. Serious students of Latin America should be grateful for *Bureaucratic Authoritarianism* regardless of the timing of publication.

The structure of the book is somewhat complex. Chapters devoted to theoretical explanation are followed by others devoted to a narrative account. The narrative account of each phase of bureaucratic authoritarianism (1966–69 and 1969–73) is followed by a chapter devoted to the analysis of quantitative indicators supporting judgments derived from the theory. Yet while such an arrangement is confusing at first glance, it has the merits of allowing the author to develop both his case study and comparative framework coherently.

The lietmotif of the work is how the contradictions within the ruling coalition supporting bureaucratic authoritarianism were an underlying cause of its collapse. The ruling coalition is composed of the military, the upper bourgeoisie, and transnational corporations (TNCs). Not only are there disparities of interest among the groups, there are significant divisions within them. (Most notably, the military is divided among paternalists, nationalists, and liberals; and the upper bourgeoisie is split between those connected with industry, commerce, and finance and those based on the agricultural export economy of the Pampas.) The mission of bureaucratic authoritarianism is to secure acceptable rates of return on capital in the present and the indefinite future. A key element of this mission is to secure the confidence of the TNCs, especially financial institutions.

As already indicated, this explanation does more than simply repeat the theoretical model that originally appeared in *Modernization and Bureaucratic Authoritarianism.* By emphasizing different features of bureaucratic authoritarianism, it seems to modify as well as refine the earlier explanation. In the latter, the author concentrated on the factors that "exhausted" import substitution industrialization (ISI) as an economic strategy and rendered populism no longer viable by the mid-1950s. The new economic task was to "deepen" industrial development by moving substantially into the field of capital goods production. This, in turn, meant increased capital accumulation and exploitation of the working class. This change in policy ruptured the populist alliance by dividing the industrial bourgeois from their erstwhile collaborators in the ISI process.

In the present book, O'Donnell focuses on politics and policies within bureaucratic authoritarianism, stressing the importance of transnational capital within the tripartite alliance. He emphasizes the importance of the reaction of TNCs and multilateral financial institutions, particularly the International Monetary Fund (IMF). This change seems to be more than a matter of perspective. It has a number of implications. If extraction of further surplus value is the key, politics—although having a transnational component—would seem to be largely domestic. In addition, the success or failure of policies could best be measured by such quantitative indicators as the absolute level and relative share of industrial production within the gross domestic product. And in promoting industrialization, the state could play a direct role through the development and expansion of state enterprises. On the other hand, if transnational factors are pivotal, the purely domestic dimension of bureaucratic authoritarian economic policy is marginalized and state intervention (since it is, by and large, contrary to the preferences of TNCs and the IMF) must be discouraged. Moreover, the creation of what might be called a "favorable business climate" becomes a better measure of success than macroeconomic indicators. The autonomy of the state, too, becomes less important. It is, now, clear that the state's autonomy is only "relative." Although it is dominant over most sectors of civil society, the state is closely linked to the upper bourgeoisie. O'Donnell's revised analysis seems more valid than the original presentation, especially given the deindustrialization of a number of bureaucratic authoritarian regimes, notably Chile and 1976–83 Argentina.

All in all, *Bureaucratic Authoritarianism* carries the discussion of military governments forward in a useful direction. O'Donnell raises the possibility (to borrow a phrase he used in regard to ISI and populism) of the "exhaustion" of the bureaucratic authoritarian model of development and explores, in some detail, the inherent contradictions within the bureaucratic authoritarian alliance. The emphasis on what might be considered psychological factors such as business confidence, political intransigence, and delegitimation makes clear that although the political agenda may be driven by a kind of economic determinism, political responses are not.

PAUL W. ZAGORSKI

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Politics in Jamaica. By Anthony J. Payne. New York: St. Martin's, 1988. 196p. $29.95.
Up the Down Escalator: Development and the International Economy—A Jamaican Case. By Michael Manley. Washington: Howard University Press, 1987. 332p. $19.95.

Jamaica, a former British colony, is of interest to social scientists because of these unique attributes: constitutional and democratic government under conditions of race and class inequalities, clientelistic violence, and an economy highly dependent on external capital transfers for the population's daily necessities. Each of these two books under review addresses different aspects of this paradox. *Politics in Jamaica,* Payne's fourth book on the Caribbean, does not provide a novel framework or a set of new ideas. Race and class inequalities, "democratic socialism," "orthodox liberal development," destabilization, and Jamaica's relations with Cuba and the Commonwealth Caribbean, are the themes of the book. They have already been exhausted in his previous publications. In fact, 8 of the 10 chapters have already appeared as journal articles and chapters in books. While the book cannot be praised for theoretical and empirical innovations, it provides a useful short overview of postindependence Jamaican politics, easily accessible in a single source.

Payne argues that the Manley government's "democratic socialism" was counterproductive as it alienated external support and dismantled the government's critical multiple-class domestic alliance. On the other hand, he argues that the Seaga government's "orthodox liberalism" resulted in severe hardships for the lower classes and the poor, compromised Jamaica's sovereignty over its national affairs and ultimately failed. He nevertheless states emphatically that "analysis of Jamaica's experience with development policy since independence suggests that this is the only road left, short of socialist revolution" (p. 10).

Politics in Jamaica lacks an in-depth treatment of state-society relations and fails to identify the main actors and their interactions with other social forces. The author argues that the democratic political structure is anchored on elite socialization into British values without taking into account the many new empirical studies showing that this is not clear (e.g., Carl Stone's *Class, Race and Political Behavior in Urban Jamaica* [1973]), pointing to how state patronage is used to guarantee multiple class support of the political system. Perhaps the most serious limitation of the book is its lack of significant analysis of the state's important role in holding the political structure together. In consequence, his claim that the state enjoys sufficient autonomy over local and international capitalist interests, enabling the government to restructure the Jamaican economy successfully is contradicted by his recognition of the strength of the local and foreign capitalists in blocking "democratic socialism" and creating a crisis for the state.

Up the Down Escalator is Prime Minister Manley's personal account, written after his humiliating defeat, of Jamaica's international relations during the 1972–80 period while he was in office. He explains the crises and breakdowns that are common in North-South relationships. *Up the Down Escalator* is more specific than *Politics in Jamaica* in that it is limited to an analysis of the role played by external capital sources in the development process in Third World nations, using a Jamaican case study. While its perspective is similar to that expressed in economist Norman Girvan's *Corporate Imperialism* (1978), it casts the Jamaican problem in a broader Third World framework with international implications and consequences. Manley, a leading Third World social thinker and activist, speaks on behalf of the poor people of the South victimized by colonialism and imperialism, appealing for a negotiated and mutually advantageous solution to externally induced problems. It is sober and thoughtful, well-written, lucid, with excellent notes, limited technical jargon and clear tables, making it accessible to readers from a wide range of backgrounds.

Manley correctly argues that the political and economic organizations that result from external dependency block Third World efforts at self-sustained development. In the Jamaican case, when "windows of opportunities" emerged (such as bauxite investment) that could provide the "up escalator" for progress, he argues, global events wreaked havoc on the economy creating the "down escalator."

The Jamaican-International Monetary Fund (IMF) experience has become somewhat of a cliché in development studies. Manley presents his version of this celebrated encounter. He challenges IMF policies in Third World environments, arguing that structural adjustment policies designed to create favorable conditions for exports and foreign investments most often lead to increased socioeconomic hardships for the working classes and the poor. He blamed the IMF's rigid commitment to capitalist orthodoxy (as well as its political machinations) for his electoral difficulties.

Manley does not advocate Jamaican or Third World "disengagement" from the international system. He believes that the Third World can break out of its dependency relationship with the North if certain reforms are adopted. For him, needed policy reforms include (1) a new international economic order that favors Third World nations, (2) the creation of Third World regional and international institutions, (3) South-South cooperation, and (4) political education to create an ideology of self-reliance within the Third World. Manley is aware that many Third World and several Scandanavian leaders agree with these reforms; but in his view, powerful forces in the North block the adoption of such recommendations and are likely to continue to do so unless public opinion forces a change in their foreign policy. Surprisingly, the book has an upbeat conclusion emerging from the author's undying faith in the peoples of the Third World: South-South cooperation may provide an alternative.

The two books reviewed are worth reading. They complement each other, although together they do not provide a complete picture of the Jamaican political economy. *Up the Down Escalator* is perhaps the more important of the two, in terms of its wider potential audience. Its most important contribution is that it helps the reader understand the difficulties of the policy-making process in international relations. While Manley is certainly correct in identifying the root causes of Jamaica's problems in its linkages with the international economic order, he seriously underestimates the role of domestic political processes in Jamaica's economic problems. The clientelist political structure is perhaps the single most important obstacle in creating a self-reliant population. Internal clientelism and external dependency are linked—one perpetuates the other. Most analysts of Jamaican politics, including Payne and Manley, fail to offer analyses linking internal political processes to external dependency relations.

CARLENE J. EDIE

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Sport and Politics: The Olympics and the Los Angeles Games. By Bill Shaikin. New York: Praeger, 1988. 101p. $32.95.

More Than a Game: Sports and Politics. By Martin Barry Vinokur. Westport: Greenwood, 1988. 208p. $37.95.

Politics and Leisure. By John Wilson. Boston: Unwin & Hyman, 1988, 200p.

In sporting circles, the 1980s are likely to be remembered as much for the political controversies that surrounded athletic performances as they are for outstanding achievements. Successive Olympic boycotts in 1980 and 1984, the Zola Budd affair, and the continuing saga concerning South African sport are but highlights of a decade during which even the most conservative observers have had to admit to the existence of a strong link between sport and politics. It has also been a period of maturity within the academic circles that concern themselves with such issues, and *Sport and Politics* by Shaikin, *More than a Game* by Vinokur and *Politics and Leisure* by Wilson are the latest contributions to what is rapidly becoming a specialist subfield within political sociology.

The titles of Shaikin's and Vinokur's works suggest generic interpretations of the links between sports and politics. While both authors develop themes with universal applications, their studies are, in fact, rather more narrowly focused. Bill Shaikin is a San Francisco Bay sports journalist who covered the 1984 Olympic Games in Los Angeles. *Sport and Politics* is his attempt to make sense of the broader context within which he found himself working, but the book is more than the armchair reflections of a news hound. In order to make sense of the Los Angeles olympiad, Shaikin first investigates the complex relationship between sport, culture, and politics in the United States and integrates his findings within a historical and contemporary review of the politicization of the Olympic movement. All this he does rather well before providing a detailed and insightful analysis of the events leading up to, including, and following the Los Angeles games. The book's major weakness is its lack of theoretical depth and weight, but in some ways this contributes to its major strength: an honest, fluid, and no-nonsense style. Shaikin is not a full-time academic; and while he is familiar with the all-American literature of social commentators such as James Michner and popular sport sociologists such as Harry Edwards, he makes no attempt to locate his work either within mainstream political and social theory or within the emergent subfield concerned with the political sociology of sport and leisure. However, in many ways this enables him to make best use of his license and experience as an investigative journalist; and a certain lack of theoretical weight is balanced with well-informed commentary and insightful, commonsense, analyses.

More Than a Game has as its subtitle *Sports and Politics* but once more tends to be rather specifically focused, this time around a comparative analysis of sport and politics in Romania, the German Democratic Republic, and (implicitly) the United States. Unlike Shaikin, Vinokur is very much constrained by a certain academic tradition, leading him to follow the pattern of Ph.D. dissertations favored by so many U.S. universities: theoretical framework, hypotheses, methodology, data analysis, discussion, and conclusions. This approach limits Vinokur's scope and constrains the interest of the reader in that most of the conclusions are given away in the first chapter, wherein the theoretical categories of structural-functionalism are adopted, more or less uncritically, as a framework for the interpretation of the "evidence" as it unfolds. The extent of the theoretical review is insufficient to justify the high status granted to the functionalist paradigm. If one of the purposes of the book is the application of grand theory, surely functionalism must be seen to contest this terrain with versions of Marxism, critical theory, and figurational sociology. Given a functionalist preference and a research design whose cornerstone is hypotheses testing, it is surprising that he relies entirely on qualitative materials and interpretations to test his hypotheses. Vinokur criticizes sports scholarship of socialist countries for being "discriptive or historical, aimed at proving that sport is a political institution" (p. 133). This precisely characterizes his own work. Nevertheless, there is some commendable detail in *More Than a Game* that should be of particular interest to those concerned with sports development in East European countries and the politics of the Olympics.

Vinokur might have got more out of his materials had he followed a more thematic and critically interpretative approach, like that employed by Wilson in *Politics and Leisure*. Of the three works, Wilson's is undoubtedly the best informed and the most scholarly, being located within a school of thought based upon the type of mellow, Marxist, cultural analysis associated with Raymond Williams and E. P. Thompson and includes the political sport and leisure sociology of, among others, John Hargreaves, John Hoberman, Richard Gruneau, John Clarke, and Charles Critcher. Wilson offers a wide-ranging analysis of the complex webbing of relations between leisure and politics with particular emphasis on sport and the politics of the state. He does this without letting the theoretical context of his study overwhelm the narrative, which is rich in historical and ethnographic detail. Essentially, Wilson views leisure in general and sports in particular on both sides of the Atlantic as terrain unevenly contested between dominant and subordinate social groups whereby, for the most part, ruling classes or bureaucratic elites generally succeed—albeit unevenly—in molding the popular leisure culture of the masses to suit their own ends. *Politics and Leisure* is the fifth in Unwin and Hyman's Leisure and Recreation Studies series; and while it is one of the best, it tends to suffer from a certain duplication, particularly in the first chapter, which goes over definitional and conceptual ground well covered earlier in the series. Also, the author has a degree of difficulty establishing clear boundaries when music halls, sexual preferences, eating habits, cruelty to animals, parks and gardens, and various versions of football are all treated as part of a common social institution, leisure.

In their different ways, all three books contribute to a developing body of knowledge in the area of sport, leisure, and politics, with

Politics and Leisure being a particularly valuable reference for anybody working in this area.

JOHN SUGDEN

University of Ulster

The Politics of Military Rule in Brazil, 1964–85. By Thomas E. Skidmore. New York: Oxford University Press, 432p. $29.95.

This book establishes Thomas Skidmore as the leading foreign analyst of contemporary Brazilian history. His *Politics in Brazil, 1930–64* (1967) examined how and why the coup d'etat of 1964 had come about. In 1967 he did not know that the period of military-controlled government would stretch to 1985, but in succeeding years he remained a careful observer of each stage of military rule and so has been able to write a sequel that faithfully captures the sense and meaning of Brazil's traumatic military era.

In twentieth-century Brazil, the growth of the military's political power coincided with the development of the republic's political institutions and traditions. In the United States the latter were in place before significant expansion of military influence, and hence civilian politicians had the tools with which to restrain it. In Brazil, the military have long been an active, nearly independent political force. Even so, in 1964, there was little reason to suspect that officers would behave differently than in previous interventions since 1930, in which they had quickly returned power to civilians. The existing literature did not provide enough information about the internal workings of the armed forces to make observers wary. After all, the coup was led by veterans of the only Latin American army to send troops to fight the Axis in World War II.

In *The Politics of Military Rule in Brazil,* Skidmore describes and explains the process that led the military to keep power and then to relinquish it gradually over 21 years. Faced with defeat in the 1965 gubernatorial elections in key states, Humberto de A. Castelo Branco sought to restrain antidemocratic officers on the Right and populist politicians on the Left with the codification of the president's (and thereby the military's) arbitrary powers at the expense of the legislature and judiciary. This gamble succeeded on the Left but gave the subsequent governments of generals Artur Costa e Silva (1967–69) and Emilio Garrastazú Médici (1969–74) the basis for authoritarian rule. The armed forces officer corps was divided between those who believed that the military should confine themselves to their professional duties and leave governing to civilian politicians and those who regarded the politicians as scoundrels who would betray the country to communism or some other menace. These latter officers won out in the military's internal struggles that gave rise to what Juan Linz labeled Brazil's "authoritarian situation." But because they could not ignore the counterweight opinions of their less-hard-line colleagues or the resistance of Brazilian society, they were not able to carry out a political institutionalization similar to Franco's Spain. Moreover, Skidmore argues, they did not attempt to eliminate entirely the trappings of liberal constitutionalism because they feared disapproval of international opinion and loss of the alliance with the United States: "They saw America as the bastion of anticommunism, an ideology that furnished the justification for their unrelenting hold on the levers of power in Brazil" (p. 158). And because Washington preached liberal democracy, the authoritarians' dependence on the United States "reinforced their contradictory position in domestic politics" (p. 158). Their concern for appearances led them to avoid a personalist dictatorship and to ensure that each successive general-president fulfilled his term and then passed power to his replacement.

Throughout, Skidmore links the political process to the economy, which in 1967–74 was setting records for rapid growth; to the workers' struggle, not to be sacrificed on the altar of Brazilian security and development; and to the increasingly active role of the Catholic Church as the voice of criticism and restraint. He is particularly good at showing how these forces supported, without compromising themselves, the liberalizing efforts of the returned Castelistas, Ernesto Geisel (1974–79) and João Figueiredo (1979–85), whose policies of "relaxation" (*distencão)* and "opening" (*abertura*) were fiercely opposed by the hard-line officers. Skidmore tells the dramatic story of how Geisel's break with the United States over atomic energy allowed him to assume the nationalist mantle and to use it to muster support among the generals to remove his insubordinate hard-line minister of the army, General Sylvio Frota. Ironically, it was Geisel, the most autocratic of the post-1964 presidents who bested the hard-liners and ensured the eventual return to democracy.

Of course, the many crises of the Figueiredo years—the worsening debt situation, the Rio-Centro bombing attempt, Figueiredo's heart attacks, and the movement for direct presidential elections—would have to be traversed before that occurred. Figueiredo was not as politically skilled as Geisel, and his failure to prevent the government party from nominating disreputable Paulo Maluf may well have been the key to Tancredo Neves's election in 1985. The victor's tragic death gave the presidency to former government party figure José Sarney. Skidmore explores the relationship between its origins and the Sarney administration's weak legitimacy.

This excellent book is required reading for those who would understand contemporary Brazil. (Its only blemish results from numerous typographical errors.)

FRANK D. MCCANN

University of New Hampshire

Bureaucracy and Political Power. By B. C. Smith. New York: St. Martin's, 1988. 263p. $40.00.

Comparing Public Bureaucracies: Problems of Theory and Method. By B. Guy Peters. Tuscaloosa: University of Alabama Press, 1988. 217p. $15.00 paper.

Each of these books discusses governmental bureaucracies as political phenemona. Each is developed from lectures. B. C. Smith's *Bureaucracy and Political Power* originated as lectures to second-year undergraduates at the University of Bath. Accordingly, each chapter summarizes, in an orderly way, the work of various theorists of bureaucracy; the book is essentially textual in the sense that it takes us on a rather extensive literature tour without introducing new ideas or research results.

Yet a definite point of view underlies the tour. The subject is bureaucracy's political power, and the object is to state that bureaucracy's political power is difficult to control. This is, of course, a well-traveled route; the interesting point is that while Smith openly argues that ideological bias often colors the way administration is treated in academic writings, he does not seem concerned with acknowledging his own biases. This is probably because they are the conventional biases, namely, that bureaucracy is inherently dangerous, elitist, and antidemocratic.

Within the bounds of theoretical summary and conventional perspective, Smith covers his material well. He starts with the problem of definitions of bureaucracy and then moves to theories of responsiveness and accountability in a liberal society. He then summarizes non-liberal theories of bureaucracy found in corporatism, Marxism, and fascism, ending with the New Class argument as manifest in socialist and postcolonial regimes. Smith next deals with three popular models of bureaucracy: bureaucratic politics, public choice, and the public encounter. He ends by exploring organizational democracy and governmental decentralization as alternatives to bureaucracy. Curiously, after writing in a consistently anti-bureaucratic tone throughout the book, in his final 1½ pages Smith notes that bureaucracy also possesses some good qualities; for instance, it produces services with some effectiveness and stands for equality of treatment. It is as if at the last minute the author suddenly drops his ideological blinders—unfortunately, too late to make the book less conventional and more provocative.

Guy Peters's *Comparing Public Bureaucracies*, originating in the Coleman Ransone lectures at the University of Alabama, also reviews a wide range of public administration writings. He also takes a rather orthodox view, which is that comparative public administration as a scholarly pursuit has failed miserably to live up to its promise. Peters does, however, give us some fresh ideas to consider and some original findings to ponder.

Peters assumes the posture of the rigorous social scientist as he assesses the field of comparative administration. He argues that bureaucracy is a political phenomenon and should be studied like other phenomena studied in political science. This means, to him, that public administration scholars should engage in explicit theoretical modeling and comparative empirical research using quantitative measures.

The author admits that in public administration research face-valid variables such as votes cast and expenditures made are often not available or measurable. He readily agrees that the task of converting comparative administration into a true social science is difficult. Nonetheless, he insists that it can and should be done and in this book sets out to point us in the correct direction. The heart chapters of the book are organized by four proposed dependent variables: numbers of public employees; numbers of public organizations; individual behavior within organizations; and relations between bureaucrats and politicians.

Several quite stimulating ideas emerge from this framework. In an analysis of the proportionate split between public and private employment in several U.S. industries, Peters notes a tendency for diminishment of extreme proportions in both sectors, leading us in the direction of "a fifty-fifty state" (p. 51). In a section on tables of organization, he points out how identifiable bureaus are hard to find in the Department of Housing and Urban Development (HUD), and suggests that HUD-style "program" structures are an important and widespread alternative to bureau-type suborganizations.

In one chapter Peters delivers a stinging critique of Herbert Kaufman's *Are Government Organizations Immortal?* (1976). He contends that Kaufman's methodology is fundamentally flawed and that his conclusions drastically understate the dynamics of federal organizational change. Peters is equally devastating in his discussion of William Niskanen and other public choice theorists of bureaucracy, arguing that legislative "sponsors" cannot be as inept as is assumed and that empirical data in the United States and United Kingdom provide almost no support for administrative behavior based on utility satisfaction.

Yet in evaluating whether Peters has in fact succeeded in pushing comparative administration very far into the hallowed precincts of respectable social science, one lays down the book with certain reservations. Indeed, the author himself admits that the book "may be extremely unsatisfying to many readers" (p. 187). In the first place, Peters is all too right in identifying the problems that keep comparative administration from achieving positivist "rigor," such as difficulties of conceptual definition, the lack of precise indicators, the problems of access, and the absence of commonly agreed-upon frameworks and terminology. In other words, Peters almost shows us himself why the task he puts to us is almost impossible. The second reason why some readers will not be satisfied is that whereas the author leads the way empirically in readily "countable" areas such as public employment and organizational succession, he gives us no new data in the areas of greatest need and difficulty, bureaucratic behavior and bureaucratic power. What he principally does here is to attack old models (such as public choice) and introduce new ones (models of bureaucrat-politician interaction).

Finally, I, at least, flatly refuse to accept the admonition that "much of the field of public administration has positively deplored the role of theory and has enshrined the role of practice" (p. 182). Public administration is not antitheoretical or even atheoretical; in fact, public administration theory as a subject is more alive and vital today than ever before. Many in the field are, however, determined to engage in theory and research beyond the positivist mode of behavioral political science. In fact, in a variety of disciplines postbehavioral scholars are questioning the very existence of objective social knowledge and hence are turning to a variety of qualitative and subjectivist methodologies. In any case, comparative administration remains an utterly seductive and essential topic for research, as Peters so ably points out. I suggest that we push on with the task using a number of methodological perspectives, including, but not limiting ourselves to, what Peters proposes.

CHARLES T. GOODSELL

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The Mafia and Clientelism: Roads to Rome in Post-War Calabria. By James Walston. New York: Routledge, 1988. 265p. $67.50.

As the locus classicus of clientelistic phenomena Italy has received more than its fair share of attention from scholars interested in exploring the complex and variegated forms of personalized linkages that, to the uninitiated observer, make Italian politics both arcane and archaic. The names of Graziano, Chubb, Allen, and Zuckerman figure prominently in the roster of analysts whose work has contributed decisively to our understanding of such phenomena. Seldom, however, has anyone attempted to deal comparatively, in time and space, with the evolution of patron-client nets in relation to the context of Italian political parties and in the light of the changing realities of regional politics. This is where Walston's book breaks new ground. Few other works in print have done a better job of joining careful conceptual analysis to in-depth empirical research. The result is an impressive effort to build upon the existing body of literature on clientelism to map out the genesis, mutations and perverse effects of patron-client exchanges in the local and regional arenas of the southern province of Calabria.

The book's theoretical frame is elegantly sketched in the opening chapter. After introducing the reader to the classic distinction between "notable clientelism" and "interventionist state clientelism," Walston offers his own definition of clientelism as a generic phenomenon: "We define clientelism as being deviant with respect to an official norm. It can only exist where there is an interplay between two sets of values" (p. 16). In other words, only where societal norms deviate from official standards of morality can the conceptual validity of clientelism be established. As the author admits, "such a definition greatly limits the scope of clientelism as an analytical tool" (p. 16). Yet its relevance to the context of Calabria can hardly be denied. The key questions raised in this first chapter—What kinds of patron-client exchanges are there in society? How do they begin? How do they continue? What currency is used in the exchange?—set the basic parameters for a critical review of alternative conceptualizations. The conclusion emerges that "clientelism may be compared with the lottery of Babylon in Borges' short story; people are conscious that on one particular turn of the wheel they are losing out to others whose numbers have given them success, but are nonetheless confident that at the next turn, fortune will be more generous. Clientelism, like the lottery, attempts to introduce order into a world which is either chaotic or weighed against one" (p. 31).

Subsequent chapters carry the analysis from a general discussion of clientelist politics at the national level ("The Clientelistic State") to a finely tuned description of the political forces at work in the regional arena ("Calabria"), ending with a detailed comparison of local-level politics in Cosenza and Reggio Calabria. Through a careful sifting of the evidence available from historical sources, open-ended interviews, and press materials the author is able to piece together a fascinating comparative portrayal of local actors and their resources, of prefects and notables, church figures and mafiosi. Not the least of the merits of Walston's analysis is that it gives a concrete, real-life dimension to the theoretical abstractions generally associated with the theme of strategems and spoils.

Equally impressive is the author's attention to the differences and similarities discernible in the operation of patron-client nets at the local level. The differential impact of party affiliations and the changes over time in the nature of public resources available to local actors are carefully delineated as are the changes in the ideological texture of the two communities. In spite of the relative prominence of the Christian democrats in Cosenza and of the socialists in Reggio Calabria, in each case the channeling of public resources into private hands remained the central feature of clientelistic strategies. But perhaps the most interesting aspect of Walston's analysis concerns the nature of the linkages between the mafia and clientage relations in Reggio Calabria. How the riots (*fatti*) of the early 1970s contributed to increased public spending on industrialization, in turn providing fresh opportunities for the mafia to gain additional political clout, is a captivating tale. Once this is said, it is only fair to add that, as a political phenomenon, the mafia is hardly the central theme of the discussion. In this respect the book's title promises a great deal more than it delivers.

The social landscape explored by Walston is, of course, a familiar one to students of clientelism. Few, however, have identified its features and analyzed its mutations with greater skill and sensitivity to comparative detail. Although there can be little doubt about the merits of the book, one is impelled to wonder whether the horrendous price tag will attract the readership that the book so obviously deserves. "Mafia morality" appears to reach out far beyond the confines of Calabria.

René Lemarchand

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INTERNATIONAL RELATIONS

Arms Sales and the U.S. Economy: The Impact of Restricting Military Exports. By William D. Bajusz and David J. Louscher. Boulder: Westview, 1988. 145p. $25.00 paper.

In recent years the U.S. Congress has balked at plans to sell sophisticated U.S. armament to certain Middle East countries. Congressional concerns revolve primarily around the threat posed to Israeli air dominance in the region by the sale of advanced combat aircraft (e.g., F-15s, F-16s, and F-18s) to Jordan, Saudi Arabia, and other member states of the Gulf Cooperation Council (GCC). What is the domestic economic impact of such restraint? How would a policy of continued restraint affect certain defense industries? These are the questions that shape the analysis in *Arms Sales and the U.S. Economy.*

Using national multipliers, Bajusz and Louscher attempt to measure the domestic economic impact of restricting arms sales to these Middle East states by assessing the direct and indirect costs in terms of income and employment forgone. Although their focus is primarily on combat aircraft, the authors also estimate potential demand among Jordan and the GCC states for main battle tanks and naval fast-attack craft.

Bajusz and Louscher use three different techniques in estimating prospective demand for these states through the year 2000: the inventory replacement approach, the multivariate analysis approach, and the trend analysis approach. The last approach has limited applicability to the gulf states due to a lack of data. The multivariate approach is potentially the most interesting, but the authors fail to provide a thorough presentation of their methodology. This failure denies other scholars the opportunity to challenge or attempt to replicate the authors' findings.

A similar complaint applies to the cost estimation model for combat aircraft. *Arms Sales and the U.S. Economy* benefited from the financial and informational resources of General Dynamics (one of the top U.S. military exporters and the prime contractor for the F-16 airframe). General Dynamics provided the authors access to "a cost estimation model to calculate illustrative cost estimates for the F-16, F-15, and F-18 based upon the prospective market for each aircraft" (p. 76). Bajusz and Louscher present per unit cost data for combat aircraft in chapter 5 based on prospective demand for Jordan and the GCC states. This information is useful for other scholars working in this area, but one would need further access to General Dynamics' model in order to apply this method to varying prospective demand for each aircraft.

Given the uncertainty over future congressional restrictions Bajusz and Louscher evaluate the economic implications resulting from four different scenarios. These scenarios range from the least restrictive case, involving only the curtailment of sales of combat aircraft to Jordan and Saudi Arabia, to the most restrictive case, incorporating restrictions on the sale of combat aircraft, main battle tanks, and naval fast-attack craft to Jordan and the entire GCC. Based on the authors' analysis, the least restrictive case could result in a loss of income to the United States of $8.54–$18.27 billion (p. 107). The most restrictive scenario could result in a loss of income of $32.87–$60.07 billion (pp. 110–11). Corresponding employment costs would vary between 270 thousand and 571 thousand man-years in the former case, and 1,027 thousand and 1,878 thousand man-years in the latter.

The authors readily acknowledge the limitations of their analysis. Chapter 7 provides a useful summary of many of these limitations as well as some of the other economic variables that could affect the economic impact of restricting arms sales. Future studies will need to incorporate a broader set of economic variables in order to refine the estimates provided by Bajusz and Louscher. For instance, do these estimates represent net losses of income, or would "means of production" be transferred into other income-producing enterprises, reducing the total loss to the community?

On a more general note, the book would have benefited from more stringent editing.

Often whole paragraphs are repeated throughout the text. In addition, some of the figures are illegible. Nevertheless, those interested in projecting future demand for arms sales and estimating the domestic economic impact of such sales will find this book useful.

ERIK K. PRATT

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International Economic Sanctions: Improving the Haphazard U.S. Legal Regime. By Barry E. Carter. New York: Cambridge University Press, 1988. $34.50.

Barry Carter's book is a welcome addition to the growing literature on economic sanctions. This literature has traditionally focused on questions of success and failure. While Carter is ultimately concerned with the effectiveness of economic sanctions, he approaches the topic from a new and refreshing angle by examining how economic sanctions may be used. To this Carter analyzes all U.S. laws pertaining to economic sanctions use. The specific focus is on the president and his power to enact different types of economic sanctions. Clearly, this look at how the United States can implement economic sanctions has important implications for their overall effectiveness. Carter proposes legal reforms he believes will increase the likelihood that U.S. sanctions will succeed.

This book can be divided into three sections. Chapters 1 and 2 provide an introduction and a review of past uses of sanctions and their effectiveness. Chapters 3–10 examine U.S. laws, emphasizing the power of the president. Chapters 11 and 12 suggest alternatives to the present U.S. legal regime. The first section is the book's weakest link. In particular, the second chapter is a bit of a disappointment. It relies too heavily on a single source, Hufbauer and Schott's 1985 book, *Economic Sanctions Reconsidered*. Indeed, at times it seems to be little more than a summary of that work. Given the size of the literature on this subject, a broader approach would have been desirable. Especially noticeable by its absence was any analysis of David Baldwin's excellent book on this subject *(Economic Statecraft,* [1985]). In particular, the evaluation of sanctions' overall effectiveness would have benefited from a discussion of Baldwin's idea of multiple goals.

Chapters 3–10 constitute the heart of the book. Here Carter provides the reader with a thorough examination of existing U.S. laws regarding a broad range of international economic activity. These chapters are well researched and largely descriptive in nature. Chapter 3 provides a detailed description of nonemergency laws regulating bilateral government programs. This is the area in which Carter considers the president's authority to be greatest and in fact increasing.

Chapters 4 and 5 examine the laws governing the use of U.S. exports and imports for foreign policy purposes. Carter contrasts the president's extensive authority to control U.S. exports with his quite-limited ability to affect imports. This makes sanctions on exports more likely despite the fact that they are sometimes more costly and less effective than import restrictions would be. Carter attributes this difference in presidential authority to the fact that import laws were traditionally created for domestic reasons (e.g., to protect U.S. industry) while export controls were established for foreign policy reasons.

Chapters 6 and 7 focus on private financial transactions and international financial institutions (principally the International Monetary Fund and World Bank). Here also, the president's authority is severely limited. The results of U.S. attempts to influence international financial institutions have been mixed at best, and Carter argues persuasively that U.S. ability to use international financial institutions for sanctions is likely to decrease as the percentage of U.S. donations relative to other nations declines.

Chapter 8 describes two remaining laws for nonemergencies, and chapter 8 discusses present laws regulating a declared national emergency. In times of national emergency, the president has sweeping powers to invoke economic sanctions. Here Carter raises questions concerning the dangers of the casual use of national emergencies and problems with effectively terminating these emergencies. In practice, congressional involvement is limited despite reformist legislation passed in the 1970s. Questions concerning the separation of powers are raised more fully in chapter 10, which examines the possibility of extrastatutory powers for the president.

In chapters 11 and 12 Carter presents his case for reforming the current U.S. legal regime. He examines the laws for imposing economic sanctions on several U.S. allies and concludes that the executive in these countries generally has broader powers than in the United States. He then proposes changes in U.S. laws regulating presidential authority in the areas of exports, imports, and private financial transactions. The thrust of Carter's recommendations is to make presidential authority more equal across the three areas. To do this he recommends legislation limiting presidential discretion over exports, and increasing presidential power to control imports and private financial transactions.

Overall, Carter's book is important reading for anyone interested in U.S. economic sanctions use. Carter's study makes several contributions to the existing sanctions literature. First he has painstakingly compiled the many and diverse U.S. laws on the subject. The thoroughness and attention to detail found in these chapters alone makes the book a valuable reference tool. Second, the focus on presidential ability to employ different types of sanctions is new and contributes to a better understanding of why the U.S. selects the policy instruments it does. This in turn has important implications for the general question of sanctions effectiveness. If Carter is correct in his assessment that "economic sanctions for foreign policy purposes are here to stay" (p. 270)—and I think he is—it is essential to move beyond the traditional focus of the literature on success and failure in the target country to examine how and why economic sanctions are used. Carter's book goes a long way toward answering the first question for the United States, and in so doing lays a foundation for those interested in researching the second.

ELIZABETH S. ROGERS

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Iran and the United States: A Cold War Case Study. By Richard W. Cottam. Pittsburgh: University of Pittsburgh, 1988. 297p. $29.95.

The cessation of hostilities between Iran and Iraq led the observers of the politics of the Persian Gulf, including me, to surmise that perhaps the Iranian revolution was at last undergoing a taming phase. Then came the Salman Rushdie incident and the fury of this revolution was once again unleashed by the Khomeini regime. Richard Cottam's book provides a background to this revolution, an insightful analysis of the rhythm of the revolution, and the future dynamics of the U.S.-Iranian relationship. Throughout the volume, the author is preoccupied with explaining the *intensity* of the encounter between the United States and Iran, a point taken for granted but not adequately explained by most studies on Iran.

The original involvement of the United States in Iran might not have been guided by a mission to dominate the domestic politics of that country. Its eventual outcome, according to Cottam, produced results sadly paralleling that of the British and the Soviet involvements of the past century. As the cold war between the United States and the USSR progressed, Iran became one of the states where the United States became heavily involved in containing the USSR. From the perspective of the domestic politics of Iran, the U.S. preoccupation with containment also became a reason for its growing intervention in that country's economy and politics. The Iranians perceived the cold war-related U.S. presence in their country as "a continuation of losing struggle against European imperialism" (p. 11).

Since Iran was largely seen as a strategic prize, the U.S. decision makers never really developed an elaborate understanding of the complexities of the Iranian domestic politics. Consequently, states Cottam, they remained largely uninformed and almost totally incapable of developing long-term strategy.

One of the major reasons why both Britain and the United States became so unpopular in Iran was the fact that their obsession to control the pace and direction of change in that country forced them to cooperate with its traditional and conservative elite and spurn the liberal intelligentsia. In fact, the latter group became the target of the successful Anglo-American coup d'etat. Given past Anglo-American predilection for the conservative elite, one can only imagine the U.S. perception of the Khomeini regime, which is at once populist and authoritarian.

U.S.-Iranian relations, notes Cottam, still appear to be dominated by a number of myths. The Iranian perspective of this myth is that "Khomeini's coming to power was part of an explicit and very clever U.S. strategy designed to contain the Soviet Union" (p. 14). The United States saw the fall of the shah as an outcome of U.S. lack of will, "not [as] a reflection of basic inadequacy in capability."

As the cold war enters a new phase in the wake of the new political thinking of Mikhail Gorbachev, both the United States and the Soviets are well advised to consider the following arguments made by Cottam. First, the emerging alterations in the U.S. perceptions of the diabolical nature of the Soviet Union seeking to control the world might also enable the United States to rethink its long-standing predilection for traditional elites in the Third World countries. In the past, this obsession caused considerable resentment toward that superpower in Iran, the Philippines, and South Korea. The Soviet withdrawal from Afghanistan and the settlement of the Angolan and the Namibian conflicts might provide persuasive evidence for the United States of the changing nature of the Soviet global involvement.

Second, in the wake of transformation of the Iranian politics into "mass politic," neither superpower might be able to bring about its preferred political change—or control events—in Iran or anywhere else in the Third World. The response of the Afghan *Mujahedeens* to the Soviet occupation of their country might be seen as an evidence of the emergence of mass politics in Afghanistan. In fact, on the basis of this and other mutations in the international political arena, Cottam makes a case for fundamental alterations in the U.S. strategies relating to Iran and the Third World.

The most unfortunate aspect of the U.S.-Iranian relationship is that the former maintains a confrontational posture toward the latter as the self-destructive fury of Iranian revolution continues to manifest its deleterious effects on the Iranian polity and economy. The heated rhetoric of the Khomeini regime concerning the Rushdie affair and the resultant break in its diplomatic ties with the European Economic Community are only the recent manifestations of this fury.

The book is comprehensive in its treatment of U.S.-Iranian relations. As one of the foremost U.S. experts of the Iranian political scene, Cottam's insightful analysis should be read and given serious consideration by the top policy makers of the Bush administration as they plunge themselves into reviewing the overall U.S. policy toward the Middle East and the Gulf.

M. E. AHRARI

Mississippi State University

The Money Doctor in the Andes: The Kemmerer Missions, 1923–1933. By Paul W. Drake. Durham: Duke University Press, 1989. 335p. $50.00.

Between 1923 and 1933, Princeton University economist Edwin Walter Kemmerer led economic missions to five Andean republics. In each his team recommended an almost identical series of monetary, fiscal, administrative, and banking reforms that were uncritically adopted and were to one degree or another followed until the depression wiped out any semblance of economic order in Latin America.

Most sectors of society in all five countries greeted Kemmerer as a virtual savior. He was careful to avoid formal links with the Department of State or any North American financial institutions. As such, he brought his "scientific" techniques to South America, where they were welcomed as a means of bringing economic development and, not incidentally, of clearing the way for much-desired loans from North American lending institutions.

Historian-political scientist Paul Drake has written a fine, thoughtful book on these missions, which he sees as both representing attempts by the Andean Republics to tie themselves into the rapidly expanding international capitalist system and as a means by which North American hegemony spread to the continent in the wake of fading European (particularly British) economic and political influence. The author also admits that some of his fascination with these missions is the degree to which they parallel (if not duplicate) the International Monetary Fund missions of today. Like the IMF operations, the Kemmerer recommendations attempted to bring order to the economies of the Latin American states with a set of suggested (required?) reforms that are presented as being of an economic and organizational nature when they are actually deeply political and represent a particular vision of a proper economy and society. The analysis in this volume falls into the neodependency, world systems, and hegemony literatures. As such, it offers an enormous amount of detail as to the specifics of these reforms and how they served to link the Latin American states into the booming world economy of the 1920s.

Although Kemmerer doubtlessly felt himself independent of the Department of State and uninfluenced by banking interests, he was invited because his advice (such as his strong adherence to the gold standard) was consistent with the interests of the Department of State and important business interests. His autonomy was strictly within ideological confines. His visits would not have been recommended or supported by either Washington or Wall Street had his advice not been of the proper variety. And on the other end of the transaction, he was primarily invited to countries because by utilizing his mission, a country was able to give the impression of financial respectability that was needed in order to get foreign loans.

The book is sensitive to which groups in each country were helped and hurt by which reforms. With a few notable exceptions, political actors in the countries were unanimous in welcoming the Kemmerer missions. Labor unions, for example, hoped that he could check the raging inflationary situations that were impoverishing working people in several of the countries. The middle class welcomed the expansion of the government bureaucracies that were needed as part of bringing order and government control to the economies. Importers wanted stable currencies to facilitate their businesses. And so on. To the extent that there came to be internal opposition, usually it was nationalist leftist political contenders or agrarian elites, who benefited less from credit arrangements than urban manufacturing interests. Of course, once the reforms were actually in place, bitter political struggles took place over the application of the laws. In light of Drake's analysis, it seems fair to observe that many Latin American governments at the time more closely resembled the Marxian model of being instruments by which the upper classes controlled and exploited the masses through clientelistic politics and corruption than benevolent representatives of the public good. Despite this, due to Kemmerer's being a supposedly neutral, nonpolitical outsider with international stature, his missions were accepted into the political process. As Drake points out, the reforms they championed were rather well known in Latin America and controversial. However, because the missions were seen as above politics, the reforms were initially welcomed. The author observes, "In actuality, both Kemmerer and the IMF [today] helped install economic policies usually desired by local elites as well as by North Americans, and transnational coalitions made these policy changes possible" (p. 7).

One unexamined question in this book is the degree to which the Latin American governments in question had any genuine choices in terms of what economic policies made sense. Drake seems to suggest they did not—alternative models of economic and political development were lacking. What Drake criticizes is countries that followed the supposed scientific advice of the Kemmerer group too rigorously and refused to adjust for economic and social forces.

Readers interested in the main arguments of the volume but not concerned about the experiences of the individual countries or the sometimes minute, detailed discussion of individual cases can get most of the author's main theoretical points and conclusions by concentrating on the provocative first and last chapters. To carefully read each individual case may require more concern with historical detail than non-Latin American experts will have.

This is an important volume that will be read for years to come. It is a model of how carefully done historical research can contribute to the building of political theory. It offers evidence of significance to the dependency and hegemony debates. Besides being well researched, it is written with an admirable clarity of style. Drake is particularly successful in his introduction and conclusion at placing his materials in a theoretical context and in showing how "Kemmererism" is alive and well today. The advice has changed little, and it is a bit frightening when the author points out that the heavy debt burdens of the time in question eventually were contributing factors to the depression of the 1930s.

MICHAEL J. FRANCIS

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Book Reviews: International Relations

Change and Stability in Foreign Policy: The Problems and Possibilities of Detente. By Kjell Goldmann. Princeton: Princeton University Press, 1988. 252p. $35.00.

An extensive but rather narrow literature on East-West detente has developed over the past 15 years. Much of it is either substantive policy analysis or traditionally historical or a combination of the two. Unlike coercive bargaining, use of deterrent threats, and use of economic sanctions, detente has not been well integrated into the theoretical literature on foreign policy processes. This book addresses that gap.

Goldmann seeks to explain policy stability amidst pressures for change. He works from two principal assumptions. First, while virtually all policies are affected by environmental disturbances, "a number of phenomena tend to block, delay, or reduce the scope of policy change, even in the presence of pressure for change" (p. 63). These 13 "stabilizers" belong to four categories: international, cognitive, domestic-political, and administrative. Second, it is easier to theorize about the sources of policy stability than the pressures for change. While the literature mainly discusses the processes that undermined detente, Goldmann focuses on the factors that could conceivably have protected the policy but did not. Although he does not draw heavily on the structural realist literature, his second assumption and research design reflect on its major limitations: international system variables alone leave a good deal of foreign policy behavior unexplained. Ten of the stabilizers are thus found at the levels of domestic polity, decision-making group, or individual decision maker.

A loose model of policy stabilization is presented. External dependence, for example, diminishes sensitivity to disturbance and negative feedback through cognitive mechanisms (causal beliefs become more consistent and central in the belief system), politically (high costs of change increase domestic support), and administratively (as links to others are bureaucratized at home). Comparable connections are made among the other stabilizers. Although stabilization at one level, especially dependence, reinforces other stabilizers, the process is "a race between the gradual buildup of stabilizers and the incidence of stress" (p. 72).

The case study compares Washington's and Bonn's relationship with Moscow during the 1970s and early 1980s. The substantive findings are unsurprising: Ostpolitik survived stress better than U.S.-Soviet detente. Goldmann's analytic conclusions are more interesting. U.S. policy was less stable than Germany's largely because the United States was less economically dependent on trade with the East and because U.S. policy makers' beliefs about detente were neither central to their goals nor free from major inconsistencies. Officials in Bonn were less troubled by these difficulties. Readers are reminded that the model is only a theoretical sketch, and the author believes that it needs substantial refinement in light of the single test presented here.

The book's major strength is to place detente in a theoretically meaningful yet policy-relevant context. As John Gaddis has emphasized ("The Evolution of U.S. Policy Goals toward the USSR in the Postwar Era," in *Gorbachev's Russia and American Foreign Policy,* [1988]), ed. Michael Mandelbaum and Seweryn Bialer, the demarcation between cooperation and competition is not always similarly perceived. This inherent difficulty was compounded by Washington's explicit "dual track" policy of detente *and* deterrence: the former assumes an adversary who will conditionally cooperate while the latter assumes one who needs constantly to be kept in check. But in Bonn's view, Moscow was somewhat less aggressive; the two tracks were more consistent. By systematically discussing a well-known case in terms of stabilizing and destabilizing forces, Goldmann contributes to foreign policy theory.

The author's theoretical claims are modest. His stabilizers, loosely connected in a rough temporal sequence, constitute a theoretical sketch of stabilization. Several areas do seem to call for further work. Goldmann suggests that political culture variables may affect political and administrative stabilization; if so, links among beliefs, values, and the historical development of institutions need further examination. In addition, since the impact of international norms and third parties was not as predicted, a cognitive approach might be called for. How statesmen *view* rules and third parties is probably more important than an objective assessment of their impact.

In sum, this book is a useful addition to the substantive literature on détente and a rich source of fresh thinking on generic foreign policy processes. Serious students of both subjects will wish to consult it.

JOSEPH LEPGOLD

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Long Cycles: Prosperity and War in the Modern Age. By Joshua S. Goldstein. New Haven: Yale University Press, 1988. 433p. $45.00 cloth, $19.95 paper.

George Santayana's familiar maxim that those who do not remember the past are condemned to repeat it has missed the mark if Joshua Goldstein's thesis is correct, for remembering the past has nothing to do with it. According to Goldstein, the past five centuries of world history have been pieced together from repeating sequences of "themes, processes, and relationships" that constitute the long cycles of his title. Two such cycles are the central concerns of this book—cycles of economic expansion and stagnation lasting about 50 years or so and longer cycles of world hegemony and hegemonic war of roughly 150 years duration. The former coincide with the so-called long waves sometimes named for the Russian economist Nikolai Kondratieff while the latter correspond to the succeeding hegemonies postulated by Modelski and Wallerstein and their respective associates.

To many social scientists the mere mention of historical cycles will evoke an uneasy image of rigidly patterned movement such as the swaying of a pendulum or the vibration of a piano wire. This view comes dangerously close to interpreting the past and future as wholly foreordained by cyclic forces. Goldstein emphatically rejects this image of social cycles as excessively mechanistic. He prefers a conceptualization that defines cycles in terms of alternating phases of varying duration rather than in lockstep synchrony to calendar time. Moreover, there is nothing mysterious or mystical about cyclical phenomena once the underlying causal dynamics are understood. After all, it can be reasoned that if common business cycles are generally accepted as a feature of standard macroeconomic theory, there is nothing in principle against cycles of longer duration.

This ambitious book, a revision of a MIT doctoral thesis, divides into three parts corresponding to Goldstein's three main tasks. The first and longest part aims to review and integrate a surprisingly large and widely scattered body of work on economic long waves as well as a somewhat more manageable literature on cycles of war and hegemony. Long and methodical literature reviews are customarily squeezed out of dissertations as they are revised for publication so it is to Yale University Press's credit that this valuable review has remained essentially intact. Especially useful are the several "geneology charts" that Goldstein constructs to help the reader quickly grasp the connections and divisions among the disparate long-cycle authors and schools.

Parts 2 and 3 contain Goldstein's own contributions to long cycle scholarship. The empirical analyses of part 2 focus on long waves, encompassing the statistical examination of war and more than 50 economic time series of varying length that together cover almost five hundred years. Eschewing the inductive approaches often applied to long-cycle detection, Goldstein devises a method of phase period analysis that employs paired t-tests to test for differences in growth rates between adjacent upswing and downswing phases of hypothesized long waves. The a priori delineation of phase turning points required by this approach is "developed by splicing together four scholars' datings." Although Goldstein argues that this hybrid base-dating scheme represents a "strong consensus" of 33 long wave analysts, one wonders if even the original four (Braudel, Frank, Kondratieff, and Mandel) would endorse his dates, let alone replicate them with their own schemes of comparable length.

The results of these analyses generally support the existence of long waves in prices, production, and war, though not always unambiguously. Goldstein uses his findings as an empirical foundation on which to build an integrative causal theory of long waves. The theoretical core of this edifice consists of a two-way causal relationship between war and production with other elements—including prices, wages, innovations, investment, social memory, and national capabilities—all interconnected via a structure of "negative feedback loops with time delays." Throughout this effort he successfully maintains a cautious balance between his recognition of the severe limitations of data and evidence on the one side and his desire to produce a coherent and plausible theoretical account on the other.

Part 3 marks a shift in both style and substance as Goldstein adopts a "structural history" approach aimed at mapping hegemony cycles in the European world system since 1500. His formulation treats the contemporary era as the fourth in a series of hegemonic periods inaugurated by the two World Wars and legitimized by Yalta, Bretton Woods, and other postwar arrangements. The United States-led period is preceeded by an era of British hegemony dating from the Congress of Vienna and Dutch hegemony from Westphalia. Such a sweeping interpretation of history is bound to engender controversy among those bold enough to undertake it. Was there a hegemon before Westphalia? If there was, who was it? (Goldstein argues for Venice.) When does Dutch hegemony give way to British? How do we distinguish hegemonic wars from others (such as the Seven Years War)? Questions such as these will continue to be hotly debated with little hope of resolution.

And what of the future? In this time of declining hegemony are we condemned once again to repeat the long cycle of hegemonic war and restructuring? Goldstein's answer is a firm *maybe.* Applying his long-wave theory to the present and immediate future suggests a possible production upturn some time in the next decade or so with a war period "danger zone" following during the period 2000–2030. Of course, Goldstein will be retired from his university post by the time of this projection. Still, he sees plenty of reason why the present era might be different enough from the past to break the cycle altogether.

The enormous scope and varied methodology of this book make it extremely difficult to summarize in a brief review. Naturally some parts are stronger than others, in part because Goldstein is a better social scientist than historian. Even so, this is a book that will stand for some time as an exemplar of long-cycle scholarship.

WILLIAM J. DIXON

University of Arizona

The European Community and South Africa: European Political Co-operation under Strain. By Martin Holland. London: Pinter, 1988. 181p. $37.50.

Besieged Bedfellows: Israel and the Land of Apartheid. By Benjamin M. Joseph. Westport, CT: Greenwood, 1988. 184p. $35.00.

As the South African state experiences growing pressures from forces both inside and outside the country, understanding the complexity of its international relations becomes increasingly important. The books by Holland and Joseph make solid, complementary contributions to such an understanding. Both are dispassionate, descriptive analyses of policies pursued by countries crucial to South Africa's future, the members of the European Community (EC) and Israel. Holland focuses on structures and processes that shape European Political Community (EPC) decisions on South Africa as well as the substance of economic relations between the Community, its member states, and the apartheid state. Joseph takes a more historical approach to the totality of interactions between Israel and South Africa.

In his examination of EPC policy objectives and outcomes, Holland includes a discussion of the four policy periods in Community-South Africa relations, economic ties between member states and South Africa, mineral dependence in Europe, the EC Code of Conduct for businesses operating under apartheid, selective sanctions begun in 1986, and aid programs for southern Africa as well as for victims of apartheid inside the country. Less central parts of the book focus on an introduction to the development of the EPC and constraints on its collective behavior, other EC-related actors involved in making or implementing policy toward South Africa, and a brief outline of descriptive models that might be used to attain greater depth of analysis. The author's emphasis on structures and processes within the Community as well as relations between member states make this work a useful case study in European politics over and beyond its contribution to a comprehension of South Africa's international relations.

Although not new or surprising, Holland's conclusions are, nonetheless, well documented and convincing. He argues that Community policy has moved forward "at the most conservative of speeds" (p. 141), culminating in voluntary economic sanctions and victims' aid programs in 1986. The EPC may have facilitated movement toward such policies in the United Kingdom and Germany, but the search for a common position actually hindered the Netherlands, Ireland, and Denmark from taking harsher measures. Such lowest-common-denominator outcomes are due to constraints within EPC processes such as "consensus rather than majoritarian outcomes; different domestic concerns; diffusion in policy-making implementation; reluctance to use a Treaty basis for joint action; the limitations of intergovernmentalism in contrast to unitary or even federal action in foreign affairs; a preference for declaratory diplomacy; and, reflexive foreign policy-making where a joint position, *per se*, is often the dominant objective" (p. 152).

Joseph's investigation is much more provocative in both the focus of its analysis and its conclusions. Joseph examines the history of political, cultural, economic, and military (including conventional, nuclear, and counterinsurgency) ties between Israel and South Africa and analyzes the similarities and differences between the internal and external situation of the two societies. Using data from news reports and other publicly available sources (including reports in the Hebrew press and South African Jewish periodicals), Joseph carefully and fully documents the extensive and deliberate building of relations between these pariah states as they each became more isolated in the international community. In the range of topics covered, this may be the most comprehensive and systematic display of Israeli–South African interactions available.

Joseph concludes that "what is commonly referred to as 'trade' between South Africa and Israel is in fact a diversified economic relationship with potential to serve as a lifeline for Pretoria in the event that Western sanctions become tighter than they are now" (p. 39). Furthermore, he suggests that military relations will continue to thrive despite United States congressional objection and scrutiny.

While crediting the Israeli government with occasional declarations of distaste for apartheid and for being concerned with the plight of Jews in South Africa, the author argues that Israeli–South African relations go beyond "realpolitik practiced by two relatively isolated states with few alternatives" (p. 89) to include common interpretations of Biblical bases for policies and behavior, friendship with twin-city agreements, and ceremonial hosting of both South African and Bantustan leaders. Whereas the same kind of amity could be demonstrated with regard to South Africa's relations to other countries, Joseph claims that the peculiarity of the Israeli connection is that both states enshrine ruling group ideologies of "*Herrenvolk* democracy at what is evidently an inauspicious time in history" (p. 122): "Israel and South Africa . . . are not and do not act like strange bedfellows. Their partnership has rested on a community of interests and complementary resources, but that is far from the entire story. As the world's two remaining political systems committed to separatist philosophies that exclude indigenous Third World majorities, they have developed an underlying sense of kinship and solidarity. This sense can only intensify in the foreseeable future and beyond, as resistance, 'iron fist' state violence and counterviolence spread in both areas" (p. 131).

At several points Joseph notes that voices critical of the alliance can be heard within both countries. He is also careful to point out (the unlikely possibility) that other data may arise to offer contending or contradictory conclusions because the whole story of the relationship cannot be told at this point due to governmental secrecy in both countries. The author makes a convincing case for his argument about why these two countries relate the way they do, but the analysis could be strengthened with more examination of the differences in their situation and their adaptive strategies. Furthermore, although Joseph states from the beginning that his examination in this work is limited to Israeli–South Africa relations rather than broader topics of anti-Semitism, colonialism, racism, and so on, one yearns for greater depth of analysis incorporating an understanding of the Jewish experience of oppression that does more than illuminate the contradictions of how victims of the Holocaust came to be entangled with so racist a regime.

JANICE LOVE

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Book Reviews: International Relations

On War and Morality. By Robert L. Holmes. Princeton: Princeton University Press, 1989. 320p. $35.00 cloth, $13.95 paper.

Paths to Peace: Exploring the Feasibility of Sustainable Peace. By Richard Smoke, with Willis Harman. Boulder: Westview, 1987. 114p. $21.50 cloth, $11.95 paper.

Can war really be eliminated? These books answer *yes,* if human beings possess sufficient commitment and faith. To generate such commitment is a principal concern of Robert Holmes, in his philosophical treatise, *On War and Morality.* Starting from the premise (which he believes militarists and pacifists alike can accept) "that it is presumptively wrong to do violence to *innocent* persons" (p. 44), Holmes reaches the conclusion that in the modern world, war—both nuclear and conventional—is presumptively wrong. Holmes does not claim that war cannot be justified. Rather (he argues) past attempts to justify it have failed; and in the absence of a convincing defense, it must be adjudged immoral.

Holmes reviews—and finds wanting—the two major schools of thought that have sought to defend war. First are the realists, who claim either that concepts of "right" and "wrong" have no meaning in international affairs (the positivistic realists) or that moral considerations should not be allowed to determine foreign policy decisions (the normative realists). His critique of normative realism, the dominant U.S. strain, is especially trenchant. These thinkers, he suggests, have misread twentieth-century history, arguing incorrectly that Wilsonian idealism led to World War II and that "unrealistic" policy was the United States' undoing in Vietnam. Worse, they have misunderstood the nature of morality itself, wrongly equating it with moralism and a blind obedience to principle that ignores the consequences of one's actions.

Having disposed of the realists, Holmes takes on the just war theorists. Rejecting the notion that just war theory has been rendered obsolete by the existence of nuclear weapons, he finds a more serious flaw. The criteria of *jus ad bellum* and *jus in bello* (by which war has traditionally been justified) are, Holmes asserts, inadequate to provide such justification. Since "war by its nature is organized violence, the deliberate, systematic causing of death and destruction," "a necessary condition of the justifiable pursuit of *any objectives* in war, by *any means* whatever . . . is that one be justified in engaging in such killing and violence in the first place" (p. 180). Holmes argues that efforts to defend the taking of innocent human lives, including the principle of double effect and the claim that some innocents may be killed in order to save others, fail to satisfy this condition.

Holmes moves from a consideration of the morality of war to an evaluation of the principal means of preventing war during the last half century: nuclear deterrence. He concludes that deterrence is irrational and hence immoral. It cannot be proved to have prevented war in the past, and the very weapons on which it relies make war possible in the future. His rejection of both war and a system of war prevention based on the threat of war leads Holmes finally to advocate nonviolence as the only acceptable mechanism for dealing with conflict. To those who condemn this approach as unworkable and dangerous, Holmes responds that such judgments are premature and that "we will never know whether there is a realistic moral alternative to violence unless we are willing . . . to explore the potential of nonviolent action" (p. 293).

Nonviolence is, in fact, one of the means of coping with conflict discussed by Richard Smoke and Willis Harman in their book, *Paths to Peace.* Described on its jacket as "a primer on thinking about peace in a nuclear age," this slender volume outlines and briefly evaluates a number of techniques that have been or might be employed to achieve peace. These range from traditional strategies, including deterrence and arms control, to such relatively new approaches as alternative defense and the delegitimation of war through attitudinal change.

Smoke and Harman are not, however, content merely to review and assess a variety of mechanisms for preventing and dealing with conflict. They also have a mission: to show people that peace is truly possible, to free them from fatalism and feelings of helplessness, and to give them a new sense of empowerment. In their last chapter, the authors contend that prospects for peace on this planet are now more favorable than at any point in history. Why? First, psychologists and other researchers have gained an unprecedented understanding of the roots of human behavior, including aggression. Second, certain social and intellectual developments strongly suggest that the world is undergoing a sea change, a transformation as fundamental as the transition from the medieval period to the modern one. Convinced that peace can be achieved if people truly believe it, Smoke and Harman seek to provide a plausible scenario by which the world might enter a "new age."

Its final chapter notwithstanding, *Paths to Peace* is essentially a survey of more or less familiar approaches to the prevention and resolution of conflict. It succeeds in distilling the essence of these techniques and in relating them to one another. Because of its brevity, however, the book fails to address the issues associated with each approach in more than the most superficial fashion. Although, supplemented by additional readings, it could serve as useful text for an introductory course in international relations, better "primers" are available.

On War and Morality also traverses some familiar terrain, particularly in its treatment of the realist school, the just war tradition, and the irrationality of nuclear deterrence. But this is a piece of genuine scholarship; much of the analysis is original and the argumentation highly sophisticated. The volume is lucid and largely free of disciplinary jargon, rendering it comprehensible to nonphilosophers. A thoughtful and thought-provoking call for pacifism in this decidedly militaristic age, *On War and Morality* deserves a wide audience.

Books that advocate nonviolence as a means of dealing with international conflict or that foresee a fundamental transformation of the global order are, of course, hardly new. Among mainstream political scientists, such works have generally been regarded as naive or utopian. Neither of these volumes promises to be different. *Paths to Peace* may convince some readers that the world stands at the brink of a new and harmonious age; but (despite their claim to the contrary) the authors' "Aquarian" perspective has not achieved widespread intellectual respectability, and the evidence presented here is unlikely to convert most doubters. *On War and Morality* is more persuasive in its criticism of war and of those who seek to defend it, but it remains difficult to imagine U.S. leaders or the public embracing disarmament and a Gandhian approach to foreign policy any time soon. Still, these books do make an important point. Substantial progress toward world peace is unlikely to occur spontaneously. Rather, it will require the determined efforts of those who believe peace can and must be attained. Excessive skepticism masquerading as realism only hinders such progress. How much skepticism is warranted? One cannot easily say. But the international system *is* undergoing dramatic changes. What today seems impossible may yet come to pass.

STEPHEN R. ROCK

Vassar College

Extended Deterrence and the Prevention of War. By Paul K. Huth. New Haven, CT: Yale University Press, 1988. 227p. $25.00.

There are two dimensions of deterrence: general versus immediate deterrence and direct versus extended deterrence. Successful general deterrence prevents open challenges and crises from occurring. Successful immediate deterrence prevents war or occupation after a crisis has already broken out. While direct deterrence concerns threats against one's own nation, extended deterrence attempts to cover protégés. Huth's book is an investigation of extended *and* immediate deterrence only. This has to be kept in mind when evaluating his findings on, for example, the lack of importance of nuclear weapons for extended-immediate deterrence. The better nuclear weapons deter at the level of general deterrence, the less opportunity there is for them to deter at the level of immediate deterrence. Whether nuclear weapons contribute to general deterrence, however, is beyond the scope of Huth's book.

Huth's data base are 58 cases of extended-immediate deterrence between 1885 and 1984. Technically, he presents a probit analysis according to which deterrence success for the defender depends positively on the immediate and the short-term balance of forces, tit-for-tat military escalation, and firm-but-flexible diplomacy but negatively on either defender intransigence or conciliation in previous conflicts between the same attacker and defender.

Some negative findings are as important as the positive ones. The long-term balance of forces, which is critical in a war of attrition, seems quite irrelevant for deterrent success. In situations of extended-immediate deterrence, at least, denial of attacker objectives looks more powerful than the capability for retaliation. Although Huth does not spell out all the policy implications of his findings, he seems to provide ammunition for those who argue the case of stronger Western conventional capabilities. Being strong enough to prevent faits accomplis does pay. Alliance linkages per se do not contribute to extended-immediate deterrence. Similarly, neither arms transfers nor trade nor the importance of the protégé to the defender seems to matter.

Concerning bargaining strategies, Huth's main message is that defenders have to calculate the impact of their actions on the value of the status quo for the attacker. While too much conciliation may obviously undercut the deterrent posture of the defender, bullying and intransigence are likely to fail because they burden the attacker with an unacceptable image of weakness if he should settle for the status quo.

Readers who distrust statistical analyses will enjoy the case studies that cover about half the pages of the book. Since the discussion of the historical cases is guided by explicit theory, it also makes for a better understanding of the propositions about military balances or bargaining strategies.

The book is highly readable, but this may have been achieved by making it less valuable to other researchers or graduate students interested in extended deterrence. The information about the coding and the statistical analyses is scarce. At the very least, the book could have benefited from an appendix giving numerical information about all of the independent variables. Some readers would also have benefited from an appendix on probit analysis.

In quantitative international politics nothing is perfect. Huth's work is no exception. I am concerned and skeptical about the validity of his measurement of immediate and short-term military balances. The limited information about the measurement of these balances (pp. 57–60) makes me conclude that Huth's balances refer to troop strength only. But we know that equipment matters; that troops from different countries do *not* fight equally well; that well-prepared defenses, initiative, and surprise are combat power multipliers. It is hard to attribute as much military naïveté to decision makers as is implied by Huth's measures. Obviously, it is extremely difficult to improve the measures for all, or even most, cases. But the recent work by Trevor N. Dupuy (*Understanding War* [1987]) points to some ways of doing better.

Necessarily, the judgmental element in the codings for military and diplomatic bargaining must be much stronger than in simple troop counts. Since we (and the coders) know the crisis outcome, it must be extremely difficult to arrive at some evaluation of bargaining strategies that is entirely independent of its effects. A conscious attempt to counterbalance the tautological temptation in coding the bargaining strategies might create new and different sources of error. Moreover, one may raise the question whether a single score is enough to describe a national bargaining strategy that may shift during a crisis. According to Huth's own account, (pp. 192–97) both the British and the German strategies before the outbreak of World War I changed over time. Here, the case study seems to raise uncomfortable questions about the validity of the codings or even the coding scheme.

Looking through the list of cases (p. 24), I am sure neither whether the list is exhaustive under the coding rules nor whether the rules themselves are adequate. Was there no case of deterrence failure in East Asia in the 1930s with Japan as the attacker and the United States as the defender? Was there no deterrence failure in *Western* Europe at the beginning of World War II? Should we not add a Cuban crisis (encompassing both the Bay of Pigs invasion and the missile crisis a year later) to the list of cases? In my view, the less-important question is, Can the exclusion of these conceivable cases be justified under the coding rules? The more important question is, How does a set of coding rules that excludes such conceivable cases affect the findings? (Admittedly, my criticism of the list of cases blurs the distinction between general deterrence and immediate deterrence).

In spite of my skepticism about various aspects of the book, Huth's work is a great book. It deals with an extremely important problem much more rigorously than anyone else has so far. It raises questions in a fruitful and meaningful way. Until somebody else does better, Huth's work has to be accepted as "the state of the art" on extended-immediate deterrence. By not reading it, you make yourself illiterate in national security affairs.

ERICH WEEDE

University of Cologne

The Collapse of the Middle Way: Senate Republicans and the Bipartisan Foreign Policy, 1948–1952. By David R. Kepley. Westport, CT: Greenwood, 1989. 197p. $37.95.

In one of the enduring rituals of American political life, every president since World War II has promised to pursue a "bipartisan" approach to the nation's foreign policy. Yet as the months pass, the dominant reality has been sharp *partisan controversy* over one or more foreign policy issues during his administration.

In *The Collapse of the Middle Way* David R. Kepley seeks to shed light on that puzzling phenomenon by undertaking a detailed examination of the period of World War II and the early postwar years. The late Truman–early Eisenhower era witnessed the high point of "bipartisanship" not only since World War II but also throughout U.S. diplomatic history. Senator Arthur H. Vandenberg (R–MI) personified the concept of bipartisan cooperation in the foreign policy field. During the heyday of bipartisanship (Vandenberg retired from the Senate in 1950), his was unquestionably the most influential voice on Capitol Hill on diplomatic issues. With President Truman and his advisers, Vandenberg believed that if the nation's diplomatic efforts were to be successful in this new and dangerous era of "internationalism," they must be based upon the maxim that "politics stops at the water's edge." To his mind, both major political parties ought to agree upon and support a *U.S.* policy reflecting the national interest. During the late Truman period, for example, Democrats and Republicans collaborated in laying the foundations for U.S. foreign policy for almost a half century thereafter. Except during wartime, the United States has never witnessed such an impressive display of interparty unity and willingness to cooperate in behalf of diplomatic objectives as occurred under the Truman and Eisenhower presidencies. (Paradoxically, this was also the era of "McCarthyism," when the U.S. society simultaneously experienced one of its most *politically divisive stages.*)

Appropriately, Kepley's account begins by taking note of the Roosevelt administration's determination to place postwar planning for the United Nations and other developments on a firm bipartisan basis. Next, the author discusses the adoption of the containment policy, the Marshall Plan, the establishment of NATO, and the military assistance program. There follows an analysis of U.S. policy toward the Chinese civil war and toward Taiwan; the movement known as "McCarthyism"; the Korean War; the "great debate" on U.S. troops in Europe; the dismissal of General MacArthur, the decline of bipartisanship, and the emergence of what the author calls "the new bipartisanship." Unfortunately, the reader is left to wonder precisely how the "new bipartisanship" differed from the old—or from outright partisan conflict in the foreign policy field.

The implicit focus of Kepley's study is the question, Why has it subsequently proved all but impossible to recreate the kind of bipartisanship witnessed in the foreign policy field during the late 1940s and early 1950s? At more than one point, Kepley underscores the *unique combination of circumstances and forces* that coalesced to produce the Vandenberg era of bipartisanship. Although he leaves the idea largely undeveloped, I believe that his explanation of the phenomenon is essentially correct. The particular conjunction of forces or policy window existing in the Truman-Eisenhower period is unlikely to be encountered again by those engaged in foreign policy decision making. In retrospect, it may be that the golden age of bipartisanship will come to be viewed as remarkable, primarily because this level of interparty cooperation in behalf of shared foreign policy goals has been witnessed neither before nor since in U.S. diplomatic history. Just as there was only one Era of Good Feelings in the nation's history, the high tide of bipartisan collaboration in the foreign policy field has occurred *only once* since World War II. A corollary of this finding is that sincere as their declamations on the subject may be, recent presidents have not had—and *are unlikely to have*—much success in restoring bipartisanship to the conduct of foreign affairs.

Upon what evidence is such a judgment based? Critical elements or factors in accounting for the extremely high level of bipartisan cooperation described here were the legacy of successful bipartisanship created during World War II; a high level of agreement among the people and their leaders concerning the dominant threat confronting the United States abroad in the early postwar period; the reality that during part of this period, one party controlled the White House while the other controlled one or both houses of Congress (a fact demanding a high degree of effective liaison between executive and legislative officials in dealing with external problems); the evident inability of Great Britain, Western Europe, or other nations to solve their problems alone or to relieve the United States of the burden of doing so; the obvious weakness of the newly established United Nations organization; a high degree of unity *among executive officials* concerning the nature of the communist threat confronting the United States and how the United States had to respond to it; and a relatively high degree of discipline and structure *within Congress*, facilitating the concept of legislative "leadership" and encouraging coherent legislative operations. Last but not least, as Kepley fully recognizes (pp. 25, 53–54, 78–79), there was the highly favorable circumstance that in most instances during this period, key executive and legislative officials were personably compatible and *were capable of working harmoniously and constructively together.* More than proved to be the case in later years, policy makers exhibited mutual respect; and in most cases they were prepared to sacrifice immediate personal or political gain for the sake of unity in foreign affairs.

Mr. Kepley is an archivist for the National Archives and teaches at Northern Virginia Community College. His research is impressive, and his treatment of the subject is authoritative, balanced, and well informed. Yet he provides few of his own insights as to why, as the years passed, it proved so difficult to discover "another Vandenberg."

CECIL V. CRABB, JR.

Louisiana State University

Minds at War. By Steven Kull. New York: Basic Books, 1988. 352p. $19.95.

Steven Kull is a former practicing psychotherapist who has turned his attention to Soviet-U.S. relations, international security issues, and nuclear weapons. *Minds at War* concentrates on the "psychological processes involved in making defense policy. More specifically it focuses on what happens as policymakers attempt to cope with the profound changes in international relations engendered by nuclear weapons—that is, nuclear reality" (p. vii).

Kull explores this theme through free-form interviews with 84 defense policy makers from the U.S. Defense Department, the Joint Chiefs of Staff, the National Security Council, the State Department (including the Arms Control and Disarmament Agency), presidential advisors, Congress, and various private think tanks and corporations. There are also two chapters reporting on, and analyzing, interviews with 30 high-ranking Soviet respondents. The discussions were tape recorded and formed the basis for later intensive analysis.

Kull identifies two central streams of thought about nuclear weapons. One of these is the "traditional stream." This current insists that "military force continues to be a viable instrument of policy between nuclear armed powers. Concepts about the significance of relative force levels and the possibility of achieving an advantageous outcome in a superpower conflict are still considered applicable in a nuclear context" (p. 9). The "adaptive stream," on the other hand, recognizes "the profound change in the role of military relations between states and the necessity for a major adaptive response" (p. 15). This change implies recognition of mutual vulnerability and the unwinnability of superpower nuclear war; emphasis on deterrence and retaliation over conventional attack and defense; and policies of arms limitation, detente and coexistence, and mutual restraint and cooperation on arsenal design and strategic defenses. Most of the people in each stream recognize, either explicitly or deep inside, the arguments and claims of the other. Indeed, most people swim in both streams, to a greater or lesser degree, at the same time. This produces deep ambivalence, unstable and dysfunctional alternation of schemata, and profound inner conflicts among decision makers.

Kull's theoretical concern is with how cognitive inconsistencies reflect, inhibit, and contribute to adaptation to the new nuclear reality. The inconsistencies go much deeper than mere ambivalence. A central incongruity involves the inappropriate "use of analogies from non-nuclear contexts" (p. 49), employing conventional war categories to understand nuclear war. Another type of inconsistency involves paradoxical thinking—statements and beliefs that appear self-contradictory. Thus, while affirming the necessity of the continuing nuclear arms race, his respondents tell him—virtually to a person—that we are doomed.

Kull's background leads him, quite properly, to reject these inconsistencies and seek to transcend them. He believes that bringing the inconsistencies to light will help overcome denial. It will reveal the underlying structure of reality and allow us to deal with it more constructively. We may not get to universal disarmament, but we can make progress toward "a regime of mutual restraint" (p. 317).

Kull is clearly on the side of the angels. His analysis is original and insightful, perhaps even brilliant. Nevertheless, his own stance, like that of his subjects, also includes denial. While his respondents tend to deny the destructive effects of the weapons they manage, Kull discounts too heavily the continuing power of traditional ways of thinking about war and peace. Eminent historians like McNeill and Tuchman have taught us that weapons are major dimensions of our heritage and that we should not be terribly optimistic about our capacities to make wise decisions about their use. In our century, the end of the long peace and the political collapse of Western Europe suggest sober reflection. We believe that reason and progress will bring us out safely to the light at the end of the tunnel. *Glasnost* and *perestroika* promise a new era. Yet the military has not withered away. It exists to threaten, and ultimately to inflict, damage, and destruction. When two armies stand face to face, we should not be surprised that they can possibly destroy each other and the societies that support them.

Kull's book is a major contribution to the self-consciousness of the age in which we live. The verbatim records of the interviews provide a unique data base for his dissection of strategic discourse and the rationale of nuclear destruction. We look away from this modern Medusa, so that we will not be turned to stone. Kull has given us a new mirror, if we wish to use it.

FRANCIS A. BEER

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Sanctity versus Sovereignty: The United States and the Nationalization of Natural Resource Investments. By Kenneth A. Rodman. New York: Columbia University Press, 1988. 403p. $45.00.

One of the most visible areas of change in North-South relations over the last 40 years has been in the politically charged relationship between raw materials investors and their host countries. Using regime analysis, part 1 of this study traces the evolution of the relationship between U.S. multinational corporations engaged in raw materials investment and Third World governments in order to assess the extent to which power has shifted from companies to their hosts. An "imposed" regime, dominated by corporate and U.S. government interests, began to erode in the late 1960s as Third World governments were able to challenge the norms of economic liberalism, international law, and the sanctity of private property rights upon which multinational corporations had based their right of access to raw materials, a right that hosts saw as an infringement of their national sovereignty. However, in spite of numerous expropriations in the 1970s, the emerging national sovereignty quasi regime has not always been advantageous to the Third World. The author convincingly demonstrates that during this period multinationals learned that they could continue to operate profitably in spite of a rapidly changing environment.

Part 2 engages in a debate between statist and radical explanations of U.S. foreign policy behavior in the area of raw materials investment. Stephen Krasner's statist and Charles Lipson's corporate preference models are tested against a wide range of expropriation cases between 1938 and 1980. The author finds that neither model can successfully explain government and corporate responses to expropriation in all of the cases. Therefore, he proceeds to examine the conditions under which each model gives adequate explanations. During the period of U.S. hegemony (1938–68), U.S. government policy was strongly influenced by corporate antiexpropriation preferences, except in cases that impinged on national security and power politics. In these cases government behavior can better be explained by the statist model.

For the period since 1968, as U.S. hegemony began to wane and regime erosion set in, Rodman claims that neither model is completely satisfactory in explaining state behavior. Postulating the state as a unitary actor is particularly inappropriate during periods of regime instability when it becomes necessary to look at bureaucratic politics to understand government responses to rapidly changing environments. By 1974, however, the author maintains, corporate accommodation to new realities, accepting rather than trying to prevent expropriation, can better be explained by pragmatic adaptation to systemic change than by either statist, corporate preference or bureaucratic interest models.

This study is an important and stimulating contribution to the literature on the political economy of North-South relations. Rodman has successfully integrated radical and statist models (both of which are frequently used in analyzing North-South relations) with regime analysis (an approach more often used in examining relations between advanced capitalist states). He demonstrates that regime analysis can profitably be used in studying an arena where there are few shared norms, conflict rather than cooperation prevails, and behavior is often coercive rather than cooperative. His final chapter contains an explanatory typology of expropriation cases going beyond those discussed in the book, thus synthesizing much of the literature on raw materials investment as well as opening up possibilities for future research.

From the perspective of the Third World, Rodman's conclusion is a depressing one: he maintains that even though host governments were successful in their nationalization efforts and therefore managed to change regime norms and rules in their favor, their relationship to the corporations and the world economy more generally continued to be a dependent one due to their need for export markets, foreign captial, and technology. Theories of hegemonic stability, more often applied to relations between advanced capitalist states, predict a diffusion of power as the relative position of the hegemon weakens. But as Rodman correctly points out, in the case of North-South relations in the era of U.S. hegemonic decline, market forces assured that power would remain in the hands of the United States and its corporations, a prophetic conclusion strongly reinforced by the subsequent evolution of North-South relations in the 1980s. As both critics and supporters of Reaganomics have affirmed, U.S. economic power with respect to the Third World remained intact, although increasingly it was exhibited through the marketplace rather than through multilateral institutions. The study's final observation that in spite of Third World challenges, liberal norms guided the U.S. approach to raw materials investment throughout the period has also been powerfully reinforced by events of the 1980s.

J. ANN TICKNER

College of the Holy Cross

The Politics of Latin American Liberation Theology: The Challenge to U.S. Public Policy. Edited by Richard L. Rubenstein and John K. Roth. Washington: Washington Institute Press, 1988. 360p. $24.95 cloth, $14.95 paper.

While liberation theology has gained much press since the successful Sandinista revolution in 1979, there has to date not appeared a book that focuses on the politics of Latin American liberation theology as it pertains to U.S. foreign and domestic policy concerns. This volume of multidisciplinary contribution attempts to correct that omission. The collection is well organized and readable and includes a helpful glossary of terms and an up-to-date bibliography on the literature of liberation theology.

The three major themes of the book are liberation theology and history, socioeconomic problems, and public policy in the United States. The essays in this volume reflect an uneven quality with some excellent pieces of research, reflection, and argumentation

(Sigmund, Hewitt, and Fleet) and others that lack those virtues (Sontag, Garrett, Roth, and Cooper).

While the book contains some lively, interesting, and at times important pieces, ultimately it disappoints for two reasons: first, it fails to give a clear and coherent account of the political significance of Latin American liberation theology. Second, and perhaps more important, given the nature of the book, there is no sustained analysis of the movement's policy implications for the United States. Thus, while the editors claim that a shared consensus of the importance of the movement animates the essays, a significant number of the essays actually call that thesis into question. Rather, some of the contributors point to the "remarkable surge of Protestant evangelicalism" (p. 130) or "grassroots fundamentalism" (p. 171) as possibly possessing more long-term political importance for the region than liberation theology (Robertson, Fleet). McCann argues that while it is not politically insignificant, liberation theology "should no longer provoke the apocalyptic hopes and fears that too often clouded our first impressions of it" (p. 283). Robertson puts it more bluntly, claiming that "its general political significance has been greatly exaggerated" (p. 131). So much for consensus. Finally, while the editors claim that the book reflects "a broad spectrum of views" regarding the merits of the movement, it is clearly biased against it. I could only identify one who clearly is in sympathy with the movement (Ellis). On the other hand, seven clearly reject the movement (Sontag, Robertson, Garrett, Belli, Roth, McCann, and Cooper).

But the major complaint stems from the weakness in, or almost total absence of, serious policy reflection. Much of the book is given to discussion, which, though interesting, is largely irrelevant to the matter at hand. Note the striking admissions by Berryman and McCann. Both essays concern the relation that may or may not exist between liberation theology and the recent letters of the U.S. bishops concerning the economy and nuclear weapons. Berryman admits that "such a question is only indirectly relevant to policymakers" (p. 248), while McCann recognizes that such discussion "may seem out of place" in a book on the politics of Latin American liberation theology (p. 266). Yet these two essays form half the section on U.S. public policy. Policy pronouncements, when present, are uniformly superficial (see Ellis, p. 67; Roth, p. 226; Cooper, p. 303). The ultimate word on the book's policy importance comes in Roth's postscript, in which he acknowledges that the essays produce no set formula for policy makers. But he claims that "collectively they do take a stand that should guide public policy" (p. 308). What stand is that? Roth states, "About the issues posed by Latin American liberation theology, no one should decide too soon or too late" (p. 308).

Two essays are notable exceptions to the problems noted above. Sigmund, after a careful historical account of the movement, makes the important argument that liberation theologians have—under pressure from the Vatican Instructions of 1984 and 1986—largely left behind their lyrical Marxism so prevalent in the 1970s in favor of a more moderated spiritual (though still liberationist) message. And Hewitt claims that the basic Christian communities (CEBs) in Brazil, which serve as the focus of hope for liberation praxis, actually reflect a greater diversity of spiritual and political conditions than is found in the literature and that what is being produced is not a radical but "a responsible, involved citizenry" (p. 151). Here is meat for policy makers.

While academic specialists and policy makers will probably find little use for the volume, it might prove a handy reference or reader in more general introductory or survey courses in the several disciplines represented. Political scientists working in the areas of political culture or religion and politics may also find it of some use.

ASHLEY WOODIWISS

Wheaton College

The Iranian Triangle: The Untold Story of Israel's Role in the Iran-Contra Affair. By Samuel Segev. New York: Free Press, 1988. 340p. $22.50.

Out of Control: The Story of the Reagan Administration's Secret War in Nicaragua, the Illegal Arms Pipeline, and the Contra Drug Connection. By Leslie Cockburn. New York: Atlantic Monthly, 1987. 287p. $18.95.

Most interpretations of the Iran-Contra affair have taken one of two forms. The conventional interpretation, consistent with the

Tower Commission Report, the Reagan administration's position, and the conservative view argues that Iran-Contra involved the Iran initiative of arms for hostages and the diversion of some arms money to the Contras. This happened because the principal policy makers, including the president, made "mistakes of omission, commission, judgment, and perspective," thus failing to manage the foreign policy process rationally (*Tower Commission Report* [1987], p. 4). The major competing interpretation, represented by the congressional Iran-Contra report and shared principally by Democrats and liberals, argues that Iran-Contra involved much more than arms to Iran for hostages and poor judgment. In addition, Iran-Contra involved a major foreign policy effort by the Reagan administration to circumvent the law and Congress in order to covertly support the Contra war in Nicaragua. Although the congressional investigation was more comprehensive than that of the Tower Commission, much information about Iran-Contra remains hidden. The two books reviewed here provide additional information and serve as important correctives to both the conservative and liberal conventional wisdoms about Iran-Contra.

Iran-Contra is really about two different initiatives, one dealing with Iran, the other with Nicaragua. In *The Iranian Triangle,* Samuel Segev, a journalist with the Israeli newspaper *Ma'ariv,* focuses on the former. Segev asserts that while the U.S. role in the initiative is well documented, Israeli involvement remains obscure. He provides a well-written and useful historical account of U.S.-Israeli-Iranian cooperation revolving around oil, intelligence, and military assistance. This cooperative relationship "began during the Eisenhower administration in the 1950s, reaching its peak two decades later during the Nixon administration. For more than 25 years, the U.S., Israel, and Iran worked together in a strategic unofficial alliance aimed at halting the Soviet Union's expansion in the Middle East and weakening its friends in the Arab world," lasting until the rise of Ayatollah Khomeini in 1979 (p. 29). This background set the stage for the renewal of Israeli arms shipments to Iran as early as 1981 and the Israeli-aided U.S. effort to supply arms to Iran in exchange for U.S. hostages. Although he provides some information from new sources, much of Segev's material is derived from the Tower Commission and congressional reports. Hence, the author and publisher oversell the "untold" nature of the book.

In *Out of Control,* Leslie Cockburn focuses on the Contra affair. A journalist for CBS News, the author's investigation of U.S. foreign policy in Central America led her to the conclusion that there was much more to the Reagan administration's effort to support the Contras than has been revealed. According to Cockburn, "the Iran-Contra scandal, so the general message of the hearings indicate, had been an aberration, the product of a 'secret government' supplanting the normal, open government. But in the light of recent U.S. history, the administration appears to have been behaving in a traditional manner in pursuing a covert foreign policy and fighting a secret war" (p. 247).

As Cockburn makes clear, the Reagan administration's efforts to create and support the Contras, which began during its very first year in office, is only the most recent phase of a long history of U.S. intervention in Nicaraguan affairs. The organizing principle of the book is that the administration—meaning virtually all high-level foreign policy officials, including President Reagan and Vice President Bush—was involved in a broad variety of activities that circumvented Congress long before the 1984 passage of the Boland Amendment cutting off all direct and indirect assistance to the Contras. Some of these activities included CIA—rather than Contra—attacks on Nicaraguan oil installations and mining of harbors; CIA advocacy of terrorist strategies, as revealed in a Contra training manual; and the secret channeling of military equipment for the Contras from the Pentagon to the CIA or cooperative third countries (such as Honduras). The administration tried to dissociate the government from these activities by creating a secret network of private individuals and groups, including former intelligence and covert operators (such as General Secord or anti-Castro Cubans in Miami) and staunch anticommunists (for example, General Singlaub and Carl "Spitz" Channel), to raise money and equipment for the Contras. These efforts intensified after the Boland Amendment was passed, and as news of these activities leaked out, the administration tried to cover them up, prompting Attorney General Meese to stop an

FBI investigation from proceeding in Miami.

The most unsettling revelation is Cockburn's contention that many of the Contras, as well as many U.S. citizens recruited to support them, were selling weapons and supplies on the black market, laundering money, and smuggling drugs into the United States. The CIA (along with other governmental officials) was aware of much of this behavior but chose to ignore it (in some cases even aiding the drug running) for the larger goal of rolling back communism. According to Cockburn, CIA involvement in covert wars and drug smuggling also occurred during the Vietnam War, the Bay of Pigs, and earlier cold war-era U.S. interventions. This anticommunist, ends-justifies-the-means mindset is most clearly revealed by an administration plan to declare martial law and suspend the Constitution if domestic dissent resulted from U.S. militar intervention in a foreign conflict. The Constitution, in fact, was ignored by the FBI in its repression of legitimate U.S. dissent against Reagan's Central American policies and in the "diversion" of money (raised by North and Channel for the Contras) to support pro-Contra candidates in congressional races.

Many people will find Cockburn's account unsettling and incredible. Nevertheless, most of the information she provides has been previously reported, and, with the use of additional interviews and public information, she provides a very comprehensive overview. At times, given the incomplete information available, she admits to speculation—for example, concerning whether the CIA was behind the La Penca bombing to assassinate Eden Pastora and whether the Reagan campaign team's efforts to prevent an "October Surprise" by Carter in 1980 was the source of the original arms shipments to Iran.

What important generalizations do these two books provide about U.S. foreign policy? First, they illustrate the continuing importance of anticommunism as a driving force in U.S. foreign policy. Second, they demonstrate the importance of a historical perspective for understanding contemporary events and continuity in U.S. national security policy. Third, they indicate the significance of private groups, transnational actors, and foreign governments in U.S. foreign policy. Policy is not simply made and implemented by the executive branch in Washington; nor do the many actors involved share the same interests. Fourth, they point to the "covert" side as an integral part of U.S. foreign policy, although we most often tend to see and hear the overt side. Fifth, they suggest that one must be skeptical when it comes to so-called bipartisan governmental investigations, since such bodies are composed of individuals who have vested interests to protect or promote. Sixth, they indicate the contradiction between the requirements of national security (such as secrecy) and the demands of democracy (such as informed citizens), demonstrating that national security concerns tend to prevail. Finally, these books illustrate the importance of domestic politics in explaining how foreign policy issues evolve and why certain interpretations eventually become the conventional wisdom.

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The Economy of Detente: The Soviet Union and West Capital. By Georges Sokoloff. Leamington Spa, W. Germany: Berg, 1987. 250p. $37.50.

Opening Up the Soviet Economy. By Jerry F. Hough. Washington: Brookings Institution, 1988. 100p. $8.95 paper.

One of the most neglected topics in international political economy and Soviet studies has been Soviet foreign economic policy. While scholars studying other parts of the world have become absorbed with an analysis of the openness or closure of national economies and the effects of protection and free trade on national and international politics, most students of Soviet politics have been silent on these issues. Thus, because such studies are rare, these books on Soviet foreign economic policy are a welcome addition to the literature on Soviet studies and international political economy.

One of the reasons for scholarly silence on this issue has been the relative isolation of the Soviet economy from international economic developments throughout the postwar period. During the last four decades, Soviet imports have never exceeded 2% of the Soviet national product. As Sokoloff's book reminds us, however, the Soviet Union has imported technology from the West for the modernization of its economy since the mid 1950s (and even earlier). And Hough argues that if Gorbachev's internal economic and social reforms are to succeed, the Soviet Union must retreat from its protectionist stance toward the international economy. Both arguments, then, are central to the debate in Soviet Studies on the relationship between foreign and domestic policy in the Soviet Union.

Sokoloff asks one central question: Why does the Soviet Union import capital goods from the West? It is not, he argues, to increase the supply of armaments, Soviet arms stocks are largely independent of the cycles that characterize internal economic actitivy and imports, and the authorities responsible for military planning have little or no contact with the Soviet officials in charge of import policy. Nor do the Soviets trade with the West to "buy peace" in order to achieve the painless conversion of the world to socialism, Soviet rhetoric on East-West trade notwithstanding. Economic imperatives, argues Sokoloff, have in fact propelled the Soviet drive to import from the West throughout the post-Stalinist era. Capital goods are imported with a dual aim: to make up for quantitative shortfalls in domestic production and to provide the quality lacking in Soviet-made products. Furthermore, in the economic sectors where Western imports represent a large proportion of total imports and investment, access to the West's advanced technology is the deciding factor in the decision to import. Technology imports can speed up the investment cycle, and more rapid return on investment is an important key to economic development. These conclusions are based primarily on statistical data correlating investment and import cycles from the 1950s to the 1970s; the book is thus useful for scholars who wish to examine the historical context in which Gorbachev's foreign economic policy reforms are unfolding.

These findings, however, are hardly surprising. Implicitly arguing that the Soviet Union is a late developer and therefore has the advantages of "backwardness" through technology imports, Sokoloff's claims differ little from those advanced by Anthony Sutton in his historical study of Western technology and Soviet economic development or more broadly by Alexander Gerschenkron in his *Economic Backwardness in Historical Perspective* (1962). Furthermore, the motives behind the "Westernization" of Soviet trade can be deduced from the standard theory of comparative advantage: a desire to stimulate investment through the saving in production costs that a reciprocal exchange in specialization will effect. Scholars like Philip Hanson, Robert W. Campbell, and Bruce Parrott have come to similar conclusions using different methodologies. Nonetheless, these conclusions challenge the central assumption guiding U.S. export control policy—that the Soviet Union imports Western technology to feed the Soviet military machine—hence their importance to the current policy reevaluation.

Opening Up the Soviet Economy is the superior work in terms of conceptual clarity and exposition of provocative ideas. Its central argument is that despite past efforts to expand trade, the Soviet Union has a "protectionist" foreign economic policy, even though the instruments of protection are administrative rather than market-oriented. Protectionism breeds inefficiency and waste and can be traced to the disease of central planning in the Soviet economy. Thus, any effort to expose the Soviet economy to international economic forces must entail radical internal decentralization. Hough argues that Gorbachev is both willing and able to do what it takes to open the Soviet economy; but the United States must respond by relaxing export controls, especially foreign policy trade restrictions.

Hough's spirited and seductive defense of Gorbachev's reform strategy, his insistence that an opening of the Soviet economy to the West is the key to the reform effort, and his conviction that the reforms will succeed are certain to be controversial. Radical economic change that subjects Soviet industry to the sting of international competition must certainly entail a change in social policy. Income distribution must become less egalitarian in order to create incentives to economize on labor. Unemployment and heightened social conflict are the likely consequences. An entrenched bureaucracy will resist the breakdown of its protective cocoon, and an ideology insisting on the immorality of the market cannot easily incorporate a Soviet opening to the vagaries of a capitalist international economy. Finally, it is still unclear whether Western firms and governments will respond to the new Soviet foreign economic policy.

According to Hough, these problems can be overcome and Gorbachev's vision will prevail.

Hough argues simply that the domestic Soviet forces in support of reform and an opening to the West are greater than the obstacles. Demographic changes pushing the Soviet population from the country to the city and from blue-collar to managerial jobs have watered down the ideological appeal of a social policy that grants privileges to blue-collar workers. The generation now in power has been exposed to Western values and is eager to incorporate Western innovations into Soviet life. The conservative opposition "has been decisively and conclusively defeated." Most important, perhaps, there is widespread recognition among Soviet elites that the Soviet Union is a declining hegemon and will not remain a great power unless it raises the technological level of its industry to meet global competition. And an opening to the West is necessary to that end. The social climate, then, is ripe for the opening of the Soviet economy.

But radical reform will not succeed, Hough argues, even in an ideal social climate, without deft leadership. And Gorbachev has demonstrated exceptional political skill. He not only consolidated his initial political power, he adopted a political strategy of incrementalism so that each step toward reform would not arouse overwhelming opposition at any one time. Each new reform effort was cloaked in the language of nationalism and patriotism. Gorbachev initially emphasized *glasnost* over economic reform, symbolizing the promise of an opening to the West. Knowing that his personal popularity would attract loyalty, he identified himself with each incremental reform, so that opposition to policy would appear to be a personal affront. With this political strategy firmly under way, Gorbachev took the key steps toward decentralizing the economy: moving toward the privatization of agriculture, legalalizing parts of the "second economy," increasing investment in industry, and gradually chipping away at the power of the economic plan.

Inextricably linked to this domestic political and economic strategy was a decision to tear down protectionist barriers to the international economy. Gorbachev changed the institutional framework of foreign economic policy, appointing to positions of power economists who favored a more open economy and attacking the monopoly position of the Ministry of Foreign Trade. According to Hough, however, the real centerpiece of Gorbachev's foreign economic policy has been the move to establish joint ventures between Soviet enterprises and Western firms. For Hough, the joint venture policy not only dramatizes the attitude change toward the outside world but demonstrates the importance of increasing exports to the success of economic reform.

Hough argues that because Gorbachev needs the West for the success of his reforms, the United States should relax export controls—for four reasons: (1) the Soviet economic opening will be primarily directed toward a responsive Europe, and continuing a technology blockade would aggravate an alliance conflict more harmful to Western security than expanded East-West trade; (2) if the United States wishes to encourage emigration, it must support Soviet integration into the international economy so that millions of Soviet citizens will travel abroad and those who wish to emigrate can use this opportunity; (3) above all, Hough claims, economic blockades have never succeeded in liberalizing communist systems; and finally, (4) he argues that the technological gains to be achieved by a more open foreign economic policy will be slow, and that the West need not fear a major technological improvement that will transform Soviet military capability.

This book raises more questions than it is able to answer. First, if the necessary technological gains will be slow, how are they to have the critical impact on exports, competition, and elite expectations that Hough predicts? Second, Hough argues that the key to Gorbachev's success is his incremental approach to reform. Gradual reforms protect against political instability and are easier to implement. Many economists, however, argue that the success of economic reform depends on a minimum *cluster* of simultaneous changes. There is thus a central tension between the introduction of new institutions that are necessary for reform but politically explosive and the retention of old institutions that maintain stability but impede reform. Hough's brief analysis does not do justice to the importance of this tension, nor does it provide us with a framework for understanding how a potential crisis of institutional change might be resolved.

Finally, the central thesis of the book is likely to be most controversial. The Soviet Union has a large domestic economy and (like the

United States), is not "dependent" on foreign trade. Thus the ratio of trade to GNP will probably continue to be relatively small even if radical reforms in foreign economic policy are successful. Indeed, the central utility of Western imports may lie in Gorbachev's need to provide consumer goods to build political support for internal economic reforms rather than in the provision of investment goods for long term economic prosperity. Thus, the impact of foreign trade on Soviet politics may prove to be different from the impact Hough predicts, and the necessity of this trade for the success of economic reform may not be as great as Hough suggests.

Nonetheless, this is a fascinating essay, certain to be of great interest to all students of Soviet politics and international political economy. Indeed, both books make an important contribution to the ongoing debate on the relationship between change in the Soviet domestic political economy and Soviet foreign policy, as well as policy debates on export restrictions.

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New Issues in International Crisis Management. Edited by Gilbert R. Winham. Boulder: Westview, 1988. 258p. $32.50.

Most students of international relations believe that a deliberate and premeditated decision to launch a nuclear war is extraordinarily unlikely, given that the United States and the Soviet Union each possess well over 10 thousand strategic nuclear weapons. Significant scholarly attention has therefore focused in recent years on other potential pathways to a nuclear conflict. *New Issues in International Crisis Management* is a useful collection of essays analyzing the prospects for successful crisis management in the late 1980s and 1990s. The authors, as editor Gilbert Winham notes, "are not optimistic about the superpowers' capacity to manage [a] serious political crisis" (p. 225). Although nuclear weapons might deter a premediated decision to start a war, these authors share a deep concern about an unintended war beginning in a crisis through some mixture of loss of control over military forces, accidents and misperceptions at the brink, and faulty decision making under stress.

A number of the essays acknowledge that the U.S. and Soviet governments have recognized the need for improved crisis management planning and procedures but argue that the resulting agreements have been inadequate to the task. The chapter by Joseph Nye and William Ury, for example, reviews the useful bilateral steps that have been taken (such as the 1972 Incidents at Sea Agreement and the 1985 Hotline facsimile upgrade) but also proposes a number of further arms control agreements (such as a superpower "incidents agreement" for accidental ground and air intrusions) and unilateral steps (such as the participation of high-level government officials in realistic crisis simulations and exercises) to reduce the risks of unmanaged escalation. Alexander George's essay examines the tacit patterns of restraint or "norms of competition" that might supplement more formal bilateral agreements to help prevent and manage superpower crises. George's analysis is not very optimistic, however, given what he sees as the severe difficulties of diplomatic communication between the superpowers in crises and "the difficulties superpowers often experience in deciding what their interests are in a timely and reliable way" (p. 83). Similarly, Ned Lebow's chapter analyzes a number of organizational and psychological impediments to rational decision making in military crisis and argues that technological improvements in the command and control system may have worsened, rather than alleviated, the problem, by shortening decision time and raising the dangerous possibility of runaway alerts caused by rapid and decentralized action and reaction cycles.

These essays are very useful, but to a large degree are reemphasizing themes that have appeared in the authors' previous writings. This is not the case in the most original contribution to the book, Janice Gross Stein's essay seeking to explain the failure of superpower crisis prevention efforts in the Middle East from 1967 to 1982. Stein appropriately approaches her findings with caution, acknowledging that there is an important, but unavoidable, bias in case selection: "We deal only with cases where one of the superpowers failed to restrain its ally from going to war . . . because it is extraordinarily difficult to identify instances where superpowers do persuade their allies to refrain from the use of force [in the Middle East]" (p.

174). Stein's conclusions are not reassuring for the prospects of superpower crisis prevention in regional conflicts. In each of the case studies Egypt and Israel resorted to the use of military force against the advice of its superpower patron, in part because the ally anticipated that the superpower could not afford to alienate or drop its client despite its uncooperative behavior. Indeed, diplomatic efforts by the superpower to prevent a regional crisis or to communicate displeasure to the local allies were not effective, as the client states either simply ignored the messages or actually used the information to manipulate the domestic politics of its patron. In each of Stein's case studies, the local ally was able to "manage the crisis manager" and go ahead with its earlier plan despite the superpower's concerns. This is an intriguing concept and should be tested against other cases of *successful and failed* crisis prevention in subsequent research.

There are two unfortunate weaknesses in the book caused by very lengthy time in production. (The chapters were originally written in July 1985 and apparently revised in September 1986; the book was finally published in April 1988). First, a number of the authors' specific proposals were overtaken by events. For example, suggestions for jointly manned Soviet-American risk reduction centers have been superceded by the agreement to create unilaterally manned centers in Washington and Moscow in September 1987. Similarly, the book's discussion of a potential agreement to ban all land-based ballistic missiles with a short time of flight is superflous after the December 1987 Intermediate Nuclear Forces treaty.

Second, and more importantly, the editor's conclusion that political hostility and shifts in the military balance in the 1980s "increased the likelihood of political confrontation between the United States and the Soviet Union" (p. 232) might well need to be revised in light of the recent thaw in superpower relations. Indeed, there may be a final irony here. In the early 1980s, when the superpower relationship was harsh and confrontational, the need for improved crisis management procedures and operational arms control was high, but the political ability to reach such agreements was low. Today there are fewer bureaucratic and political obstacles to such agreements but also less perceived need to improve the prospects for successful crisis management. It therefore remains to be seen whether the Soviet Union and the United States will take advantage of the opportunities for better crisis management procedures and improved operational arms control agreements in an era in which the urgency for such negotiations is less apparent.

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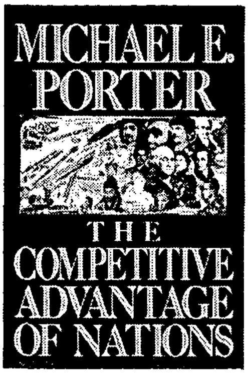

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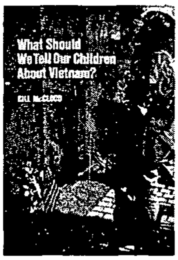

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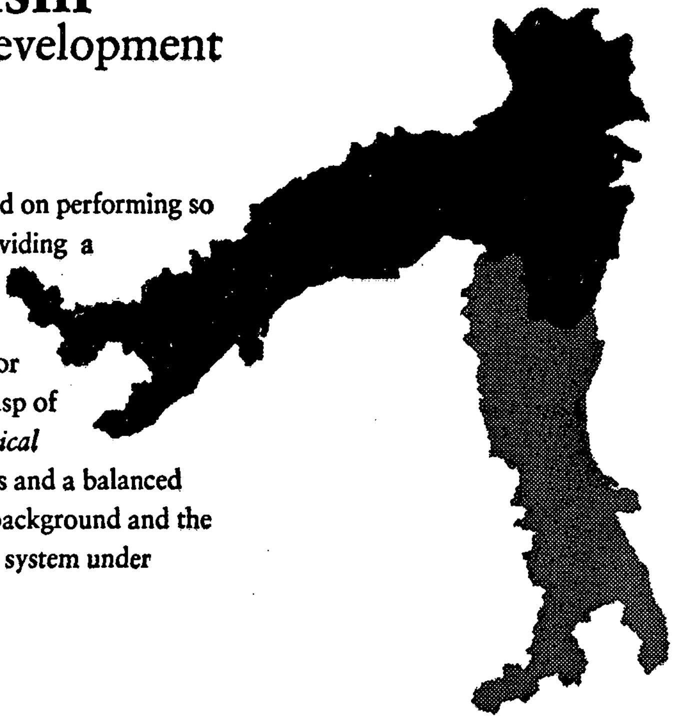

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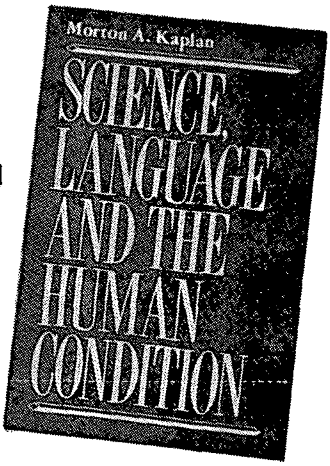

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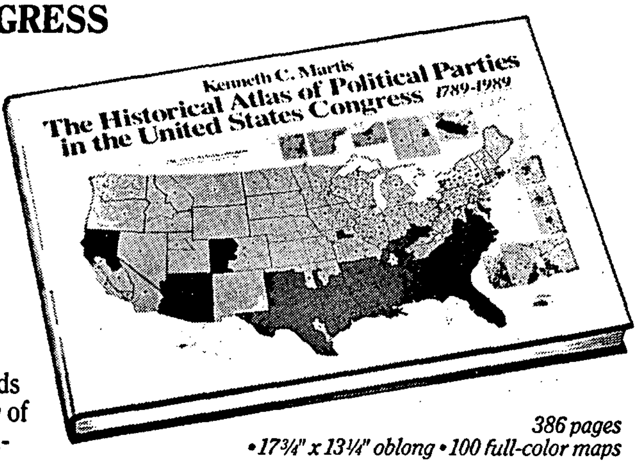

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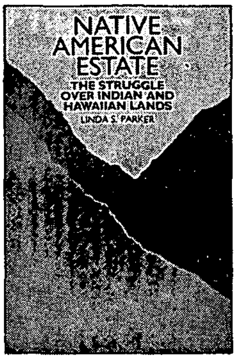
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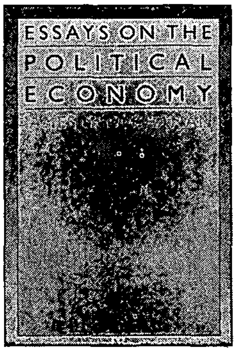
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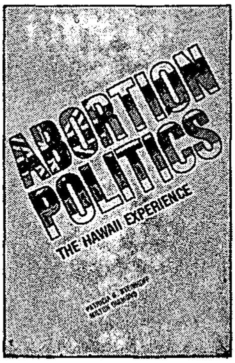
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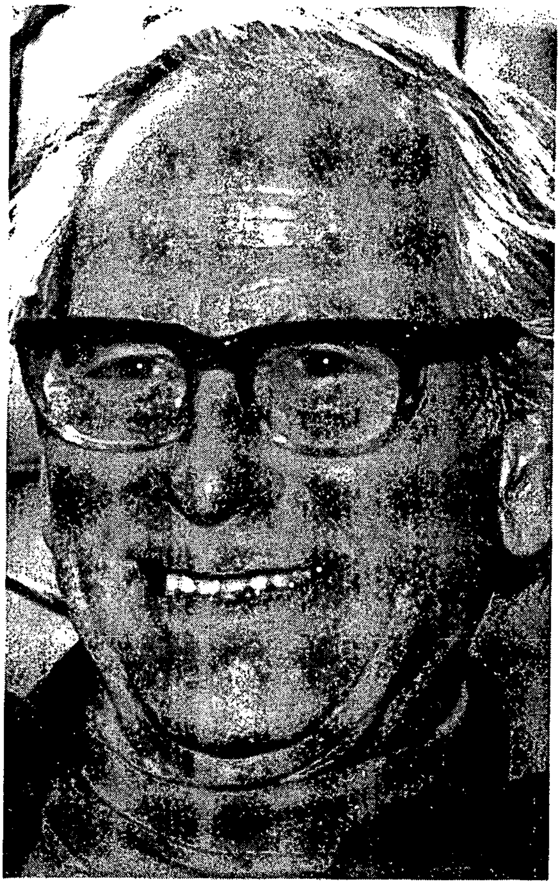

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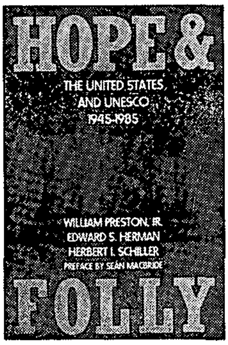
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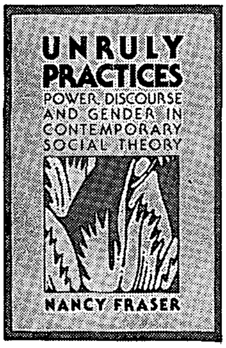
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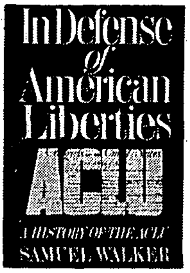
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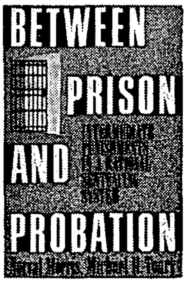
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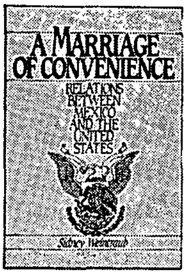
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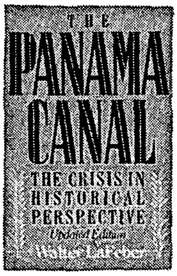
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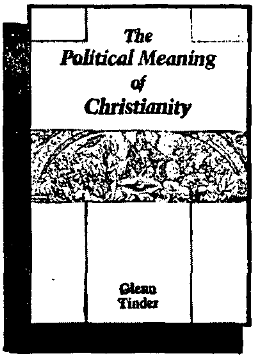

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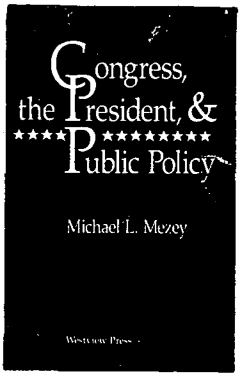

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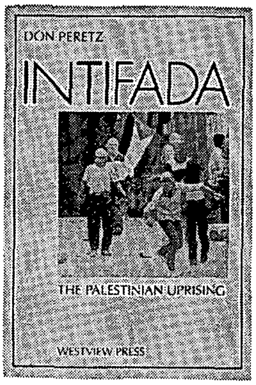

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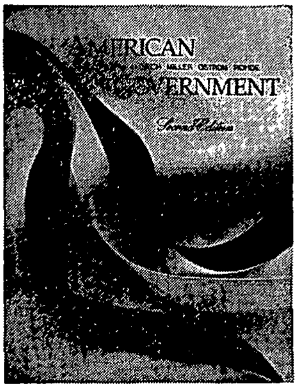

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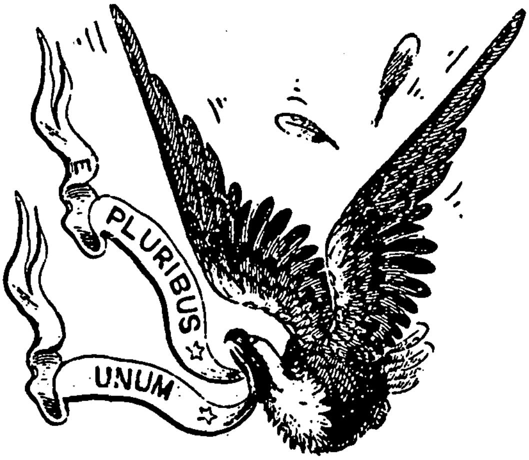

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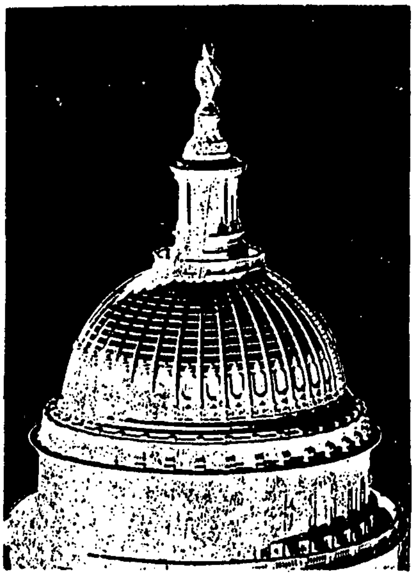

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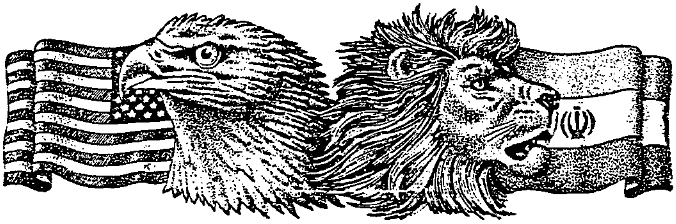

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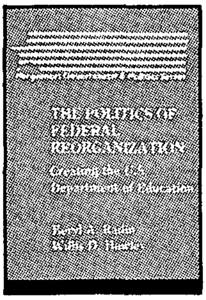
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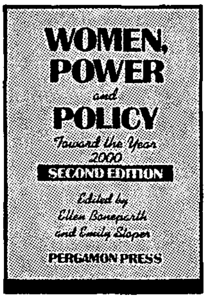

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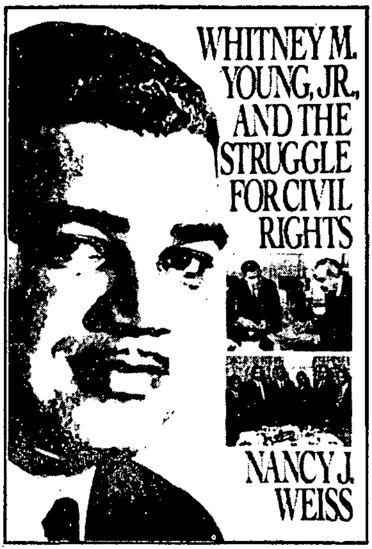

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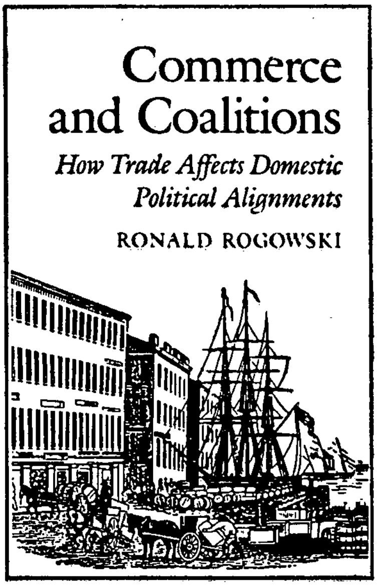

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